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OP E R U M N E W T O N I .

T O M U S Q U A R T U S .

Vol. IV.

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IS A A C I N E W T O N I

O P E R A

QUÆ EXSTANT OMNIA.

COMMENTARIIS ILLUSTRABAT

SAMUEL HORSLEY, LL. D. R. S. S.

REVERENDO ADMODUM IN CHRISTO PATRI

ROBERTO EPISCOPO LONDINENSI A SACRIS.

T O M. IV.

L O N D I N I:
EXCUDEBAT JOANNES NICHOLS.

M D C C L X X X I I.

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O P T I C S:

OR, A

T R E A T I S E

OF THE

REFLECTIONS, REFRACTIONS,
INFLECTIONS AND COLOURS

OF

L I G H T.

VOL. IV.

A

ADVERTISEMENT I.

PART of the ensuing Discourse about Light was written at the desire of some Gentlemen of the Royal Society, in the year 1675, and then sent to their Secretary, and read at their Meetings, and the rest was added about twelve Years after to complete the Theory; except the Third Book, and the last Proposition of the Second, which were since put together out of scattered Papers. To avoid being engaged in Disputes about these matters, I have hitherto delayed the printing, and should still have delayed it, had not the importunity of Friends prevailed upon me. If any other papers writ on this subject are got out of my hands, they are imperfect; and were perhaps written before I had tried all the Experiments here set down, and fully satisfied myself about the Laws of Refractions and Composition of Colours. I have here published what I think proper to come abroad, wishing that it may not be translated into another language without my consent.

The Crowns of Colours, which sometimes appear about the Sun and Moon, I have endeavoured to give an account of; but for want of sufficient Observations leave that matter to be farther examined. The subject of the Third Book I have also left imperfect, not having tried all the Experiments which I intended when I was about these matters, nor repeated some of those which I did try, until I had satisfied myself about all their circumstances. To communicate what I have tried, and leave the rest to others for farther enquiry, is all my design in publishing these papers.

In a Letter written to Mr. Leibnitz in the year 1679, and published by Dr. Wallis, I mentioned a method by which I had found some general Theorems about squaring Curvilinear Figures, or comparing them with the Conic Sections, or other the simplest Figures with which they may be compared. And some years ago I lent out a Manuscript containing such Theorems; and having since met with some things copied out of it, I have on this occasion made it public, prefixing to it an Introduction, and subjoining a Scholium concerning that method. And I have joined with it another small Tract concerning the Curvilinear Figures of the Second Kind, which was also written many years ago, and made known to some friends, who have solicited the making it publick.

I. N.

April 1, 1704.

ADVERTISEMENT II.

IN this Second Edition of these Opticks I have omitted the Mathematical Tracts published at the end of the former Edition, as not belonging to the subject. And at the end of the Third Book I have added some Questions. And to shew that I do not take Gravity for an Essential Property of Bodies, I have added one Question concerning its Cause, chusing to propose it by way of a Question, because I am not yet satisfied about it for want of Experiments.

July 16, 1717.

I. N.

T H E

T H E
F I R S T B O O K
O F
O P T I C S.

P A R T I.

MY design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by reason and experiments: in order to which I shall premise the following Definitions and Axioms.

DEFINITIONS.

DEFIN. I.

By the Rays of Light I understand its least parts, and those as well successive in the same lines, as contemporary in several lines. For it is manifest that Light consists of parts both Successive and Contemporary; because in the same place you may stop that which comes one moment, and let pass that which comes presently after; and in the same time you may stop it in any one place,

DEFINITIONS.

place, and let it pass in any other. For that part of Light which is stoppt cannot be the same with that which is let pass. The least Light, or part of Light, which may be stoppt alone without the rest of the Light, or propagated alone, or do or suffer any thing alone, which the rest of the Light doth not or suffers not, I call a Ray of Light.

DEFIN. II.

Refrangibility of the Rays of Light, is their disposition to be refracted or turned out of their way, in passing out of one transparent Body, or Medium, into another. And a greater or less Refrangibility of Rays, is their disposition to be turned more or less out of their way in like incidences on the same Medium. Mathematicians usually consider the Rays of Light to be lines reaching from the luminous body to the body illuminated; and the refraction of those Rays to be the bending or breaking of those lines in their passing out of one Medium into another. And thus may Rays and Refractions be considered, if Light be propagated in an instant. But, by an argument taken from the Equations of the times of the Eclipses of *Jupiter's Satellites*, it seems that Light is propagated in time, spending in its passage from the Sun to us about seven minutes of time: and therefore I have chosen to define Rays and Refractions in such general terms as may agree to Light in both cases.

DEFIN. III.

Reflexibility of Rays, is their disposition to be reflected or turned back into the same Medium from any other Medium, upon whose surface they fall. And Rays are more or less reflexible, which are turned back more or less easily. As if Light pass out of Glass into Air, and, by being inclined more and more to the common surface of the Glass and Air, begins at length to be totally reflected by that surface; those sorts of Rays which, at like incidences, are reflected most copiously, or by inclining the Rays begin soonest to be totally reflected, are most reflexible.

DEFIN.

DEFINITIONS.

DEFIN. IV.

The Angle of Incidence is that Angle, which the line described by the incident Ray contains with the Perpendicular to the reflecting or refracting surface at the point of Incidence.

DEFIN. V.

The Angle of Reflexion or Refraction, is the Angle which the line described by the reflected or refracted Ray containeth with the Perpendicular to the reflecting or refracting surface at the point of Incidence.

DEFIN. VI.

The Sines of Incidence, Reflexion, and Refraction, are the Sines of the Angles of Incidence, Reflexion, and Refraction.

DEFIN. VII.

The Light whose Rays are all alike Refrangible, I call Simple, Homogeneous and Similar; and that whose Rays are some more Refrangible than others, I call Compound, Heterogeneous and Dissimilar. The former Light I call Homogeneous, not because I would affirm it so in all respects; but because the Rays, which agree in Refrangibility, agree at least in all those their other properties, which I consider in the following Discourse.

DEFIN. VIII.

The Colours of Homogeneous Lights, I call Primary, Homogeneous and Simple; and those of Heterogeneous Lights, Heterogeneous and Compound. For these are always compounded of the colours of Homogeneous Lights; as will appear in the following Discourse.

AXIOMS

A X I O M S.

A X. I.

The Angles of Reflexion, and Refraction, lie in one and the same Plane with the Angle of Incidence.

A X. II.

The Angle of Reflexion is equal to the Angle of Incidence.

A X. III.

If the refracted Ray be turned directly back to the Point of Incidence, it shall be refracted into the Line before described by the incident Ray.

A X. IV.

Refraction out of the rarer Medium into the denser, is made towards the Perpendicular; that is, so that the Angle of Refraction be less than the Angle of Incidence.

A X. V.

The Sine of Incidence is either accurately or very nearly in a given ratio to the Sine of Refraction.

Whence if that proportion be known in any one inclination of the incident Ray, it is known in all the inclinations, and thereby the Refraction, in all cases of incidence on the same refracting body, may be determined. Thus if the Refraction be made out of Air into Water, the sine of Incidence of the Red Light is to the sine of its Refraction as 4 to 3. If out of Air into Glass, the Sines are as 17 to 11. In Light of other Colours the sines have other proportions: but the difference is so little that it need seldom be considered.

Suppose, therefore, that *rs* [in *fig. 1.*] represents the Surface of stagnating Water, and that *c* is the point of Incidence, in which any Ray, coming in the Air from *a* in the Line *ac*, is reflected or refracted; and I would know whither this Ray shall go after Reflexion or Refraction: I erect upon the surface of the Water, from the point of Incidence, the Perpendicular *cp*, and produce it downwards to *q*, and conclude by the first Axiom,

om, that the Ray after Reflexion and Refraction, shall be found AXIOMS. somewhere in the Plane of the Angle of Incidence *acp* produced. I let fall therefore upon the Perpendicular, *cp*, the sine of Incidence *ad*; and if the reflected Ray be desired, I produce *ad* to *b*, so that *db* be equal to *ad*, and draw *cb*. For this Line *cb* shall be the reflected Ray; the angle of Reflexion *bcp* and its sine *bd* being equal to the angle and sine of Incidence, as they ought to be by the second Axiom. But if the refracted Ray be desired, I produce *ad* to *h*, so that *dh* may be to *ad* as the sine of Refraction to the sine of Incidence, that is (if the Light be red) as 3 to 4; and about the Center *c*, and in the Plane *acp*, with the Radius *ca* describing a Circle *abe*, I draw parallel to the Perpendicular *cpq*, the Line *he* cutting the Circumference in *e*, and joining *ce*, this line *ce* shall be the line of the Refracted Ray. For if *ef* be let fall perpendicularly on the line *pq*, this line *ef* shall be the sine of Refraction of the Ray *ce*, the angle of Refraction being *ecq*; and this sine *ef* is equal to *dh*, and consequently in proportion to the sine of Incidence *ad* as 3 to 4.

In like manner, if there be a Prism of Glass (that is, a Glass bounded with two equal and parallel triangular ends, and three plain and well polished sides, which meet in three parallel lines running from the three angles of one end to the three angles of the other end) and if the Refraction of the Light in passing cross this Prism be desired: let *acb* [in *fig. 2.*] represent a plane cutting this Prism transversly to its three parallel lines, or edges, there where the Light passeth through it; and let *de* be the Ray incident upon the first side of the prism *ac*, where the Light goes into the glass; and by putting the proportion of the sine of Incidence to the sine of Refraction as 17 to 11, find *ef* the first refracted Ray. Then taking this Ray for the incident Ray upon the second side of the Glass, *bc*, where the Light goes out, find the next refracted Ray *fg*, by putting the proportion of the sine of Incidence to the sine of Refraction as 11 to 17. For if the sine of Incidence out of Air into Glass be to the sine of Refraction as 17 to 11, the sine of Incidence out of Glass into Air must on the contrary be to the sine of Refraction as 11 to 17, by the third Axiom.

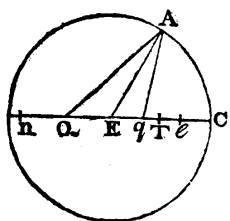
AXIOMS.

Much after the same manner, if $ACBD$ [in *fig. 3.*] represent a Glass spherically convex on both sides (usually called a *Lens*, such as is a Burning-glass, or Spectacle-glass, or an Object-glass of a Telescope) and it be required to know how Light falling upon it from any lucid point Q shall be refracted; let QM represent a Ray falling upon any point M of its first spherical surface ACB , and by erecting a perpendicular to the Glass at the point M , find the first refracted Ray, MN , by the proportion of the sines 17 to 11. Let that Ray in going out of the Glass be incident upon N , and then find the second refracted Ray, Nq , by the proportion of the sines 11 to 17. And after the same manner may the Refraction be found, when the Lens is convex on one side and plane or concave on the other, or concave on both sides.

A X. VI.

Homogeneous Rays which flow from several points of any Object, and fall perpendicularly, or almost perpendicularly, on any reflecting or refracting plane or spherical Surface, shall afterwards diverge from so many other points, or be parallel to so many other lines, or converge to so many other points, either accurately or without any sensible error. And the same thing will happen, if the Rays be reflected or refracted successively by two or three or more plane or spherical Surfaces.

The point from which Rays diverge, or to which they converge, may be called their *Focus*. And the Focus of the incident Rays being given, that of the reflected or refracted ones



(^a) IMAGINE that rays, emitted from the point Q , fall upon the spherical surface at A , and are reflected to q , and join EA . Then the angles EAQ , EAg will be equal; and of consequence QE will be to Eq as QA to Aq . And this analogy will subsist, wherever in the reflecting surface the point A be placed. Let A therefore approach to c ; and in the evanescence of the arc CA , QA becomes ultimately equal to QC , and qA to qc . Therefore QC is ultimately to qc as QE as qE . In cQ produced take qz equal to QC , and from cz cut off ce equal to Eq . Then since QC is to qc as QE to qE ; the sum of QC and qE , that is, cE , will be to the sum of qc and qE , that is, to EC , as QC to qc . (Elem. v. 12.) Also the difference of QC and qE , that is EQ , will be to the difference of qc and qE , that is to eq , as QC to qc . Therefore cE is to EC as EQ to eq (Elem. v. 11.) and of consequence $\frac{1}{2}cE$ to $\frac{1}{2}EC$ or ET to $\frac{1}{2}eq$. But cE is equal to twice ET together with twice EQ . Therefore EQ is $\frac{1}{2}eq$. Therefore TE , TC , Tq , ultimately when A coincides with c , are continually proportional. Hence the truth of the Newtonian construction is manifest.

may

may be found by finding the Refraction of any two Rays, as AXIOMS. above; or more readily thus.

Cas. 1. Let ACB [in *fig. 4.*] be a reflecting or refracting plane, and Q the Focus of the incident Rays, and Qqc a perpendicular to the plane. And if this perpendicular be produced to q , so that qc be equal to QC , the point q , shall be the Focus of the reflected Rays. Or if qc be taken on the same side of the Plane with QC , and in proportion to QC as the sine of Incidence to the sine of Refraction, the point q shall be the Focus of the refracted Rays.

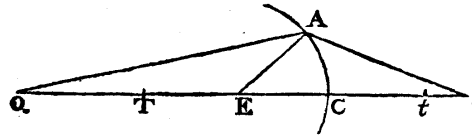
Cas. 2. Let ACB [in *fig. 5.*] be the reflecting surface of any Sphere whose center is E . Bisect any radius thereof (suppose EC) in T , and if in that radius, on the same side the point T , you take the points Q and q , so that TQ , TE , and Tq , be continual proportionals, and the point Q be the Focus of the incident Rays, the point q shall be the Focus of the reflected ones (^a).

Cas. 3. Let ACB [in *fig. 6.*] be the refracting surface of any Sphere whose center is E . In any radius thereof, EC , produced both ways take ET and ct equal to one another, and severally in such proportion to that radius, as the lesser of the sines of Incidence and Refraction hath to the difference of those sines. And then if in the same line you find any two points Q and q , so that TQ be to ET as Et to Tq , taking Tq the contrary way from t which TQ lieth from T , and if the point Q be the Focus of any incident Rays, the point q shall be the Focus of the refracted ones (^b).

And

(^b) IMAGINE rays, emitted from Q , to fall upon the spherical surface at A , and to be refracted to q , and join EA . Then is QAE the angle of Incidence, and qAE the angle of Refraction. Let i denote the sine of the former, and that of the latter. Now in the triangle QAE , QA is to QE as the sine of E to 1. By permutation QA is to the sine of E as QE to 1, or as the rectangle $QE \times qE$ to the rectangle $i \times qE$. Again in the triangle qAE , the sine of E is to qA as qE to 1. By permutation the sine of E is to qA as qE to 1, or as the rectangle $qE \times QE$ to the rectangle $qE \times qE$. But since QA is to the sine of E as $QE \times qE$ to $i \times qE$; and the sine of E to qA as qE to $QE \times qE$.

Therefore (ex æquo perturbatè) QA is to qA as $QE \times qE$ to $i \times qE$. But because TE is to EC as 1 to $r-1$, therefore, inverting and compounding, TC is to TE as r to 1. Therefore $r \times QE : i \times qE = TC \times QE : TE \times qE$; and $QA : qA = TC \times QE : TE \times qE$. And this analogy will obtain, wherever in the refracting surface the point A be placed. Let A therefore approach to c ; and since in the evanescence of the arc CA , QA and qA become



B 2

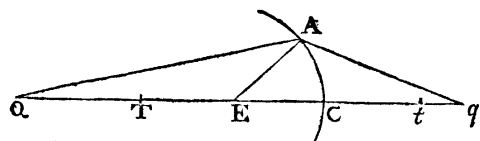
AXIOMS.

And by the same means the Focus of the Rays after two or more Reflexions or Refractions may be found.

Caf. 4. Let ACBD [in *fig. 7.*] be any refracting Lens, spherically convex, or concave, or plane on either side; and let CD be its axis (that is the line which cuts both its surfaces perpendicularly, and passes through the centers of the spheres) and in this Axis produced let F and f be the Foci of the refracted Rays found as above, when the incident Rays on both sides the Lens are parallel to the same Axis; and upon the diameter rf, bisected in E, describe a circle. Suppose now that any point Q be the Focus of any incident Rays. Draw QE cutting the said circle in T and t, and therein take tq in such proportion to tE, as tE or TE hath to TQ. Let tq lie the contrary way from t which TQ doth from T, and q shall be the Focus of the refracted Rays, without any sensible error; provided the point Q be not so remote from the Axis, nor the Lens so broad, as to make any of the Rays fall too obliquely on the refracting surfaces.

And by the like operations may the reflecting or refracting surfaces be found, when the two Foci are given; and thereby a Lens be formed, which shall make the Rays flow towards or from what place you please (c).

So then the meaning of this Axiom is, that if Rays fall upon any plane or spherical surface or lens, and before their incidence flow from or towards any point Q, they shall after Reflexion or Refraction flow from or towards the point q found by the foregoing rules. And if the incident rays flow from or towards several points Q, the reflected or refracted rays shall flow from or towards so many other points q found by the same rules. Whether the reflected or refracted rays flow from or towards the point q is easily known by the situation of that point.



become ultimately equal to qc and ge; therefore ultimately, when A coincides with c, $qc : ge :: tc : te$. Therefore the point q being given, q will be determined, by taking cq of such length, that qc may bear to qc a proportion compounded of the proportion of ge to qe , and the given proportion of tc to te . But this is effected by the Newtonian Construction. For tq being to Et , or its equal ct , as Et , or its

point be on the same side of the reflecting or refracting surface AXIOM. or lens with the point Q, and the incident rays flow from the point Q, the reflected flow towards the point q, and the refracted from it; and if the incident rays flow towards Q, the reflected flow from q, and the refracted towards it. And the contrary happens when q is on the other side of that surface.

A X. VII.

Wherever the rays, which come from all the points of any Object, meet again in so many points, after they have been made to converge by reflexion or refraction, there they will make a Picture of the object upon any white body on which they fall.

So if PR [in *fig. 3.*] represent any Object without doors, and AB be a lens placed at a hole in the window-shut of a dark chamber, whereby the rays that come from any point Q of that object are made to converge and meet again in the point q; and if a sheet of white paper be held at q for the light there to fall upon it: the Picture of that Object, PR, will appear upon the paper in its proper shape and colours. For as the light which comes from the point Q goes to the point q, so the light which comes from other points, P and R, of the object, will go to so many other correspondent points p and r (as is manifest by the sixth Axiom); so that every point of the Object shall illuminate a correspondent point of the Picture; and thereby make a Picture like the Object in shape and colour, this only excepted, that the picture shall be inverted. And this is the reason of that vulgar experiment of casting the species of objects from abroad upon a wall or sheet of white paper in a dark room.

In a like manner, when a man views any object PQR [in *fig. 8.*]

equal ct to tq ; by consequence tq and ct together, that is qc , will bear to ct and tq together, that is to qc , the proportion of tc to te . (Elem. v. 12.) But tc bears to tq the proportion compounded of the proportions of tc to te , and te to tq . And the proportion of te to tq is the same which ge bears to qe . For since qg is to te as Et to tq , by composition $qg : te :: tq : qt$. And by permutation $qg : ge :: te : tq$. Therefore the proportion of tc to tq , being compounded of the proportions of tc to te and te to tq , is compounded of the proportions of tc to te , and ge to qe . Therefore the proportion of qc to qc , being the same with that of tc to tq , as hath been shewn, is compounded of these same proportions. Whence the truth of Newton's Construction is manifest.

(c) Vide Lect. Opt. Part I. Sect. iv. Prop. xxix, xxx, xxxii, xxxiii.

the

CAUSE OF
VISION.

the light, which comes from the several points of the object, is so refracted by the transparent skins and humours of the Eye, (that is by the outward coat *EFG* called the *Tunica Cornea*, and by the crystalline humour *AB* which is beyond the pupil *mk*) as to converge and meet again at so many points in the bottom of the Eye, and there to paint the Picture of the Object upon that skin (called the *Tunica Retina*) with which the bottom of the eye is covered. For Anatomists, when they have taken off from the bottom of the eye that outward and most thick coat called the *Dura Mater*, can then see, through the thinner coats, the pictures of objects lively painted thereon. And these pictures propagated by motion along the fibres of the Optick Nerves into the brain, are the cause of Vision. For accordingly as these pictures are perfect or imperfect, the object is seen perfectly or imperfectly. If the eye be tinged with any colour (as in the disease of the *Jaundise*) so as to tinge the pictures in the bottom of the eye with that colour, then all objects appear tinged with the same colour. If the humours of the eye by old age decay, so as by shrinking to make the *cornea* and coat of the *crystalline humour* grow flatter than before, the light will not be refracted enough; and, for want of a sufficient refraction, will not converge to the bottom of the eye, but to some place beyond it; and by consequence paint in the bottom of the eye a confused picture, and according to the indistinctness of this Picture the Object will appear confused. This is the reason of the decay of sight in old men, and shews why their sight is mended by spectacles. For those convex glasses supply the defect of plumpness in the eye, and, by encreasing the refraction, make the rays converge sooner, so as to convene distinctly at the bottom of the eye, if the Glass have a due degree of convexity. And the contrary happens in short-sighted men, whose eyes are too plump. For the refraction being now too great, the rays converge and convene in the eyes before they come at the bottom; and therefore the picture made in the bottom, and the vision caused thereby, will not be distinct, unless the object be brought so near the eye, as that the place where the converging rays convene may be removed to the bottom; or that the plumpness of the eye be taken off, and the
refractions

refractions diminished by a concave glass of a due degree of concavity; or, lastly, that by age the eye grow flatter till it come to a due figure: for short-sighted men see remote objects best in old age, and therefore they are accounted to have the most lasting eyes.

A X. VIII.

An Object seen by Reflexion or Refraction, appears in that place, from whence the rays, after their last reflexion or refraction, diverge in falling on the Spectator's Eye.

If the Object *A* [in *fig. 9.*] be seen by Reflexion of a Looking-glass *mn*, it shall appear, not in its proper place *A*, but behind the glass at *a*; from whence any rays *AB*, *AC*, *AD*, which flow from one and the same point of the object, do, after their reflexion made in the points *B*, *C*, *D*, diverge in going from the glass to *E*, *F*, *G*, where they are incident on the spectator's eyes. For these rays do make the same picture in the bottom of the eyes, as if they had come from the object really placed at *a*, without the interposition of the looking-glass; and all Vision is made according to the place and shape of that picture.

In like manner the Object *D* [in *fig. 2.*] seen through a Prism, appears not in its proper place *D*, but is thence translated to some other place *d*, situated in the last refracted ray, *FG*, drawn backward from *F* to *d*.

And so the Object *Q* [in *fig. 10.*] seen through the Lens *AB*, appears at the place *q*, from whence the rays diverge in passing from the lens to the eye. Now it is to be noted, that the Image of the object, at *q*, is so much bigger or lesser than the Object itself at *Q*, as the distance of the image at *q*, from the lens *AB*, is bigger or less than the distance of the object at *Q* from the same lens. And if the Object be seen through two or more such convex or concave glasses, every Glass shall make a new Image, and the Object shall appear in the place and of the bigness of the last Image. Which consideration unfolds the Theory of Microscopes and Telescopes. For that Theory consists in almost nothing else, than the describing such glasses, as shall make the last Image of
any

AXIOMS.

any object as distinct and large and luminous, as it can conveniently be made.

I have now given in Axioms and their Explications the sum of what hath hitherto been treated of in Opticks. For what hath been generally agreed on I content myself to assume under the notion of Principles, in order to what I have farther to write. And this may suffice for an introduction to readers of quick wit and good understanding not yet versed in Opticks: although those who are already acquainted with this science, and have handled Glasses, will more readily apprehend what followeth.

P R O P O -

P R O P O S I T I O N S.

P R O P. I. T H E O R. I.

Lights which differ in Colour, differ also in degrees of Refrangibility.

The Proof by Experiments.

Exper. 1. I took a black oblong stiff paper terminated by parallel sides, and, with a perpendicular right line drawn cross from one side to the other, distinguished it into two equal parts. One of these parts I painted with a Red colour, and the other with a Blue. The paper was very black, and the colours intense and thickly laid on, that the phænomenon might be more conspicuous. This paper I viewed through a Prism of solid Glass, whose two sides, through which the light passed to the eye, were plane and well polished, and contained an angle of about sixty degrees: which angle I call the Refracting Angle of the Prism. And whilst I viewed it, I held it and the prism before a window, in such manner that the sides of the paper were parallel to the prism, and both those sides and the prism were parallel to the horizon, and the cross line was also parallel to it: and that the light, which fell from the Window upon the paper, made an angle with the paper, equal to that angle which was made with the same paper by the light reflected from it to the Eye. Beyond the prism was the wall of the chamber, under the window, covered over with black cloth; and the cloth was involved in darkness, that no light might be reflected from thence, which in passing by the edges of the paper to the eye, might mingle itself with the light of the paper, and obscure the phænomenon thereof. These things being thus ordered, I found that if the Refracting Angle of the Prism be turned upwards, so that the

Lights differing in Colour differ in Refrangibility.

Lights differ-
ing in Colour

paper may seem to be lifted upwards by the refraction, its Blue half will be lifted higher by the refraction than its Red half. But if the Refracting Angle of the Prism be turned downward, so that the paper may seem to be carried lower by the refraction, its Blue half will be carried something lower thereby than its Red half. Wherefore, in both cases, the Light, which comes from the Blue half of the paper through the prism to the eye, does, in like circumstances, suffer a greater refraction than the Light which comes from the Red half, and by consequence is more Refrangible.

Illustration. In the eleventh figure, MN represents the window, and DE the paper terminated with parallel sides, DJ and HE, and by the transverse line FG distinguished into two halves: the one, DG, of an intensely Blue colour, the other, FE, of an intensely Red. And BACcab represents the prism, whose Refracting Planes, ABba and Acca, meet in the edge of the Refracting Angle aa. This edge aa being upward, is parallel both to the horizon and to the parallel edges of the paper, DJ and HE; and the transverse line FG is perpendicular to the plane of the window: and de represents the Image of the paper seen by refraction upwards, in such manner that the Blue half, DG, is carried higher to dg than the Red half FE is to fe, and therefore suffers a greater refraction. If the edge of the Refracting Angle be turned downward, the Image of the paper will be refracted downward, suppose to dε; and the Blue half will be refracted lower to dγ than the Red half is to εε.

Exper. 2. About the aforesaid paper, whose two halves were painted over with Red and Blue, and which was stiff like thin paste-board, I lapped several times a slender thread of very black silk; in such manner, that the several parts of the thread might appear upon the colours, like so many black lines drawn over them, or like long and slender dark shadows cast upon them. I might have drawn black lines with a pen, but the threads were smaller and better defined. This paper, thus coloured and lined, I set against a wall perpendicularly to the horizon, so that one of the colours might stand to the right hand, and the other to the left.

left. Close before the paper, at the confine of the colours below, I placed a candle, to illuminate the paper strongly: for the experiment was tried in the night. The flame of the candle reached up to the lower edge of the paper, or a very little higher. Then, at the distance of six feet and two or three inches from the paper, upon the floor I erected a glass lens four inches and a quarter broad, which might collect the rays coming from the several points of the paper, and make them converge towards so many other points, at the same distance of six feet and one or two inches on the other side of the lens; and so form the Image of the coloured paper upon a white paper placed there; after the same manner that a lens at a hole in a window casts the Images of Objects abroad upon a sheet of white paper in a dark room. The aforesaid white paper, erected perpendicular to the horizon and to the rays which fell upon it from the lens, I moved sometimes towards the lens, sometimes from it, to find the places where the images of the Blue and Red parts of the coloured paper appeared most distinct. Those places I easily knew by the images of the black lines, which I had made by winding the silk about the paper. For the images of those fine and slender lines (which by reason of their blackness were like shadows on the colours) were confused and scarce visible, unless when the colours on either side of each line were terminated most distinctly. Nothing therefore, as diligently as I could, the places where the images of the Red and Blue halves of the coloured paper appeared most distinct, I found, that where the Red half of the paper appeared distinct, the Blue half appeared confused; so that the black lines drawn upon it could scarce be seen; and on the contrary, where the Blue half appeared most distinct, the Red half appeared confused, so that the black lines upon it were scarce visible. And between the two places, where these images appeared distinct, there was the distance of an inch and a half: the distance of the white paper from the lens, when the image of the Red half of the coloured paper appeared most distinct, being greater by an inch and an half than the distance of the same white paper from the lens, when the image of the Blue half appeared most

Lights differ-
ing in Colour
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most distinct. In like Incidences therefore of the Blue and Red upon the lens, the Blue was refracted more by the lens than the Red, so as to converge sooner by an inch and a half, and therefore is more Refrangible.

Illustration. In the twelfth figure, DE signifies the coloured paper, DG the Blue half, FE the Red half, MN the lens, HJ the white paper in that place where the Red half with its black lines appeared distinct, and bi the same paper in that place where the Blue half appeared distinct. The place bi was nearer to the lens MN, than the place HJ, by an inch and an half.

Scholium. The same things succeed, notwithstanding that some of the circumstances be varied: as in the first Experiment, when the prism and paper are any ways inclined to the horizon, and in both when coloured lights are drawn upon very black paper. But in the description of these experiments, I have set down such circumstances, by which either the phenomenon might be rendered more conspicuous, or a novice might more easily try them, or by which I did try them only. The same thing I have often done in the following Experiments: concerning all which this one admonition may suffice. Now from these Experiments it follows not, that all the light of the Blue is more refrangible than all the light of the Red: for both lights are mixed of rays differently refrangible, so that in the Red there are some rays not less refrangible than those of the Blue, and in the Blue there are some rays not more refrangible than those of the Red: but these rays, in proportion to the whole light, are but few, and serve to diminish the event of the Experiment, but are not able to destroy it. For if the Red and Blue colours were more dilute and weak, the distance of the Images would be less than an inch and a half; and if they were more intense and full, that distance would be greater, as will appear hereafter. These Experiments may suffice for the Colours of Natural Bodies. For in the Colours made by the Refraction of Prisms this Proposition will appear, by the Experiments which are now to follow in the next Proposition.

P R O P. II. T H E O R. II.

The Light of the Sun consists of rays differently Refrangible.

The Proof by Experiments.

Exper. 3. In a very dark chamber, at a round hole, about one-third part of an inch broad, made in the shut of a window, I placed a glass Prism, whereby the beam of the sun's light, which came in at the hole, might be refracted upwards toward the opposite wall of the chamber, and there form a coloured Image of the sun. The axis of the prism (that is, the line passing through the middle of the prism from one end of it to the other end parallel to the edge of the refracting angle) was in this and the following Experiments perpendicular to the Incident Rays. About this axis I turned the prism slowly, and saw the refracted light on the wall, or coloured image of the sun, first to descend, and then to ascend. Between the descent and ascent, when the image seemed stationary, I stopped the prism, and fixed it in that posture, that it should be moved no more. For in that posture the refractions of the light at the two sides of the refracting angle, that is at the entrance of the rays into the prism, and at their going out of it, were equal to one another^(d). So also in other Experiments, as often as I would have the refractions on both sides the prism to be equal to one another, I noted the place, where the image of the sun, formed by the refracted light, stood still between its two contrary motions, in the common period of its progress and regress; and when the image fell upon that place, I made fast the prism. And in this posture, as the most convenient, it is to be understood that all the prisms are placed in the following Experiments, unless where some other posture is described. The prism therefore being placed in this posture, I let the refracted light fall perpendicularly upon a sheet of white paper at the opposite wall of the chamber, and observed the figure and dimensions of the Solar Image formed on

(d) Vid. Lect. Opt. Part I. § 10. and Prop. xxv and xxvi.

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the paper by that light. This image was oblong and not oval, but terminated with two rectilinear and parallel sides, and two femicircular ends. On its sides it was bounded pretty distinctly, but on its ends very confusedly and indistinctly, the light there decaying and vanishing by degrees. The Breadth of this image answered to the sun's diameter, and was about two inches and the eighth part of an inch, including the penumbra. For the image was eighteen feet and an half distant from the prism, and at this distance that breadth, if diminished by the diameter of the hole in the window-shut, that is by a quarter of an inch, subtended an angle at the prism of about half a degree, which is the sun's apparent diameter. But the Length of the image was about ten inches and a quarter, and the length of the rectilinear sides about eight inches; and the refracting angle of the prism, whereby so great a length was made, was 64° . With a less angle the Length of the image was less, the Breadth remaining the same. If the prism was turned about its axis, that way which made the rays emerge more obliquely out of the second refracting surface of the prism, the image soon became an inch or two longer, or more; and if the prism was turned about the contrary way, so as to make the rays fall more obliquely on the first refracting surface, the image soon became an inch or two shorter. And therefore in trying this experiment, I was as curious as I could be in placing the prism, by the above-mentioned rule, exactly in such a posture, that the refractions of the rays, at their emergence out of the prism, might be equal to that at their incidence on it. The prism had some veins running along, within the glass, from one end to the other, which scattered some of the sun's light irregularly, but had no sensible effect in increasing the length of the coloured spectrum. For I tried the same experiment with two other prisms with the same success. And particularly with a prism which seemed free from such veins, and whose refracting angle was $62\frac{1}{4}^\circ$, I found the length of the image $9\frac{1}{4}$ or 10 inches at the distance of $18\frac{1}{2}$ feet from the prism, the breadth of the hole in the window-shut being $\frac{1}{4}$ of an inch, as before. And because it is easy to

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commit a mistake in placing the prism in its due posture, I repeated the experiment four or five times, and always found the length of the image that which is set down above. With another prism, of clearer glass and better polish, which seemed free from veins, and whose refracting angle was $63\frac{1}{2}^\circ$, the length of this image, at the same distance of $18\frac{1}{2}$ feet, was also about 10 inches, or $10\frac{1}{8}$. Beyond these measures, for about $\frac{1}{4}$ or $\frac{1}{3}$ of an inch, at either end of the spectrum, the light of the Clouds seemed to be a little tinged with Red and Violet; but so very faintly, that I suspected that tincture might either wholly, or in great measure, arise from some rays of the spectrum scattered irregularly by some inequalities in the substance and polish of the glass, and therefore I did not include it in these measures. Now the different magnitude of the hole in the window-shut, and different thickness of the prism, where the rays passed through it, and different inclinations of the prism to the horizon, made no sensible changes in the Length of the image. Neither did the different matter of the prisms make any: for in a vessel made of polished plates of glass, cemented together in the shape of a prism and filled with water, there is the like success of the experiment according to the quantity of the refraction. It is farther to be observed, that the rays went on in right lines from the prism to the image; and therefore at their very going out of the prism had all that inclination to one another from which the length of the image proceeded, that is the inclination of more than two degrees and an half. And yet according to the Laws of Optics vulgarly received, they could not possibly be so much inclined to one another. For let EG [in fig. 13.] represent the window-shut, F the hole made therein, through which a beam of the sun's light was transmitted into the darkened chamber, and ABC a triangular imaginary plane, whereby the prism is feigned to be cut transversely through the middle of the light. Or if you please, let ABC represent the prism itself, looking directly towards the spectator's eye with its nearer end: and let xy be the sun, MN the paper upon which the solar image or spectrum is cast, and pr the image itself; whose sides, towards x and y, are rectilinear and parallel, and ends, towards p and r femicircular.

circular. $YKHP$ and $XLJT$ are two rays; the first of which comes from the lower part of the sun to the higher part of the image, and is refracted in the prism at K and H ; and the latter comes from the higher part of the sun to the lower part of the image, and is refracted at L and J . Since the refractions on both sides the prism are equal to one another, that is the refraction at K equal to the refraction at J , and the refraction at L equal to the refraction at H , so that the refractions of the incident rays at K and L taken together are equal to the refractions of the emergent rays at H and J taken together: it follows, by adding equal things to equal things, that the refractions at K and H taken together, are equal to the refractions at J and L taken together; and therefore the two rays, being equally refracted, have the same inclination to one another after refraction, which they had before; that is the inclination of half a degree answering to the sun's diameter. For so great was the inclination of the rays to one another before refraction. So then, the Length of the image PT would, by the rules of vulgar Optics, subtend an angle of half a degree at the prism, and by consequence be equal to the breadth vw ; and therefore the image would be round ^(c). Thus it would be, were the two rays $XLJT$ and $YKHP$, and all the rest which form the image $pwrv$, alike refrangible. And therefore seeing by experience it is found that the image is not round, but about five times longer than broad, the rays, which going to the upper end P of the image suffer the greatest refraction, must be more refrangible than those which go to the lower end r , unless the inequality of refraction be casual.

This image or spectrum PT was coloured, being Red at its least refracted end r , and Violet at its most refracted end P , and Yellow, Green and Blue in the intermediate spaces. Which agrees with the first Proposition, that lights which differ in Colour do also differ in Refrangibility. The length of the image in the foregoing Experiments I measured from the faintest and outmost Red at one end, to the faintest and outmost Blue at the other end, excepting only a little penumbra, whose breadth scarce exceeded a quarter of an inch, as was said above.

^(c) Vide Lect. Opt. Part I. § 4—9.

Exper. 4. In the sun's beam which was propagated into the room through the hole in the window-shut, at the distance of some feet from the hole, I held the prism in such a posture, that its axis might be perpendicular to that beam. Then I looked through the prism upon the Hole, and turning the prism to and fro about its axis, to make the image of the hole ascend and descend; when between its two contrary motions it seemed stationary, I stopped the prism, that the refractions of both sides of the Refracting Angle might be equal to each other, as in the former Experiment. In this situation of the prism, viewing through it the said hole, I observed the Length of its refracted image to be many times greater than its Breadth; and that the most refracted part thereof appeared Violet, the least refracted Red, the middle parts Blue, Green, and Yellow in order. The same thing happened when I removed the prism out of the Sun's light, and looked through it upon the Hole shining by the light of the Clouds beyond it. And yet if the refraction were done regularly, according to one certain proportion of the sines of incidence and refraction, as is vulgarly supposed, the refracted image ought to have appeared round.

So then, by these two Experiments it appears, that in equal Incidences there is a considerable inequality of Refractions. But whence this inequality arises, whether it be that some of the incident rays are refracted more and others less, constantly, or by chance; or that one and the same ray is by refraction disturbed, shattered, dilated, and as it were split and spread into many diverging rays, as *Grimaldo* supposes, does not yet appear by these Experiments, but will appear by those that follow.

Exper. 5. Considering therefore, that if in the third Experiment the image of the sun should be drawn out into an oblong form, either by a dilatation of every ray, or by any other casual inequality of the refractions, the same oblong image would, by a second refraction made sideways, be drawn out as much in Breadth by the like dilatation of the rays, or other casual inequality of the refractions sideways; I tried what would be the effects of such a second refraction. For this end I ordered all things as in the third Experiment; and then placed a second prism

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immediately after the first, in a cross position to it, that it might again refract the beam of the sun's light, which came to it through the first prism. In the first prism this beam was refracted upwards, and in the second sideways. And I found, that by the refraction of the second prism the Breadth of the image was not increased; but its superior part, which in the first prism suffered the greater refraction and appeared Violet and Blue, did again in the second prism suffer a greater refraction than its inferior part, which appeared Red and Yellow; and this without any dilatation of the image in Breadth.

Illustration. Let s [in *fig.* 14] represent the sun; F the hole in the window; ABC the first prism; DE the second prism; γ the round image of the sun, made by a direct beam of light when the prisms are taken away; PT the oblong image of the sun, made by that beam passing through the first prism alone, when the second prism is taken away; and pt the image made by the cross refractions of both prisms together. Now if the rays, which tend towards the several points of the round image, γ , were dilated and spread by the refraction of the first prism, so that they should not any longer go in single lines to single points, but that every ray being split, shattered, and changed from a linear ray to a superficies of rays diverging from the point of refraction, and lying in the plane of the angles of incidence and refraction, they should go in those planes to so many lines reaching almost from one end of the image PT to the other; and if that image should thence become oblong: those rays, and their several parts tending towards the several points of the image PT , ought to be again dilated and spread sideways by the transverse refraction of the second prism, so as to compose a four-square image, such as is represented at π . For the better understanding of which, let the image PT be distinguished into five equal parts PQK , $KQRL$, $LRSN$, $MSVN$, NVT . And by the same irregularity that the orbicular light γ is, by the refraction of the first prism, dilated and drawn out into a long image PT ; the light PQK , which takes up a space of the same length and breadth with the light γ , ought to be, by the refraction of the second prism, dilated and drawn out into the long image πqkp ; and the
light

light $KQRL$ into the long image $kqrl$; and the lights $LRSN$, $MSVN$, NVT , into so many other long images $lrsn$, $msvn$, nvt ; and all these long images would compose the four-square image π . Thus it ought to be, were every ray dilated by refraction, and spread into a triangular superficies of rays diverging from the point of refraction. For the second refraction would spread the rays one way, as much as the first doth another, and so dilate the image in Breadth as much as the first doth in Length. And the same thing ought to happen, were some rays casually refracted more than others. But the event is otherwise. The image PT was not made broader by the refraction of the second prism, but only became oblique, as it is represented at pt ; its upper end P being by the refraction translated to a greater distance than its lower end T . So then the light which went towards the upper end, P , of the image, was (at equal incidences) more refracted in the second prism, than the light which tended towards the lower end T ; that is the Blue and Violet, than the Red and Yellow: and therefore was more refrangible. The same light was, by the refraction of the first prism, translated farther from the place γ to which it tended before refraction; and therefore suffered, as well in the first prism as in the second, a greater refraction than the rest of the light, and by consequence was more refrangible than the rest, even before its incidence on the first prism.

Sometimes I placed a third prism after the second, and sometimes also a fourth after the third, by all which the image might be often refracted sideways: but the rays, which were more refracted than the rest in the first prism, were also more refracted in all the rest, and that without any dilatation of the image sideways: and therefore those rays, for their constancy of a greater refraction, are deservedly reputed more refrangible.

But that the meaning of this experiment may more clearly appear, it is to be considered that the rays, which are equally refrangible, do fall upon a circle answering to the sun's disk. For this was proved in the third Experiment. By a circle I understand not here a perfect geometrical circle; but any orbicular figure, whose length is equal to its breadth, and which, as to sense, may seem circular. Let therefore AG [in *fig.* 15] repre-

sent the circle, which all the most refrangible rays, propagated from the whole disque of the sun, would illuminate and paint upon the opposite wall, if they were alone; EL the circle, which all the least refrangible rays would, in like manner, illuminate and paint, if they were alone; BH, CJ, DK, the circles, which so many intermediate sorts of rays would successively paint upon the wall, if they were singly propagated from the sun in successive order, the rest being always intercepted; and conceive that there are other intermediate circles without number, which innumerable other intermediate sorts of rays would successively paint upon the wall, if the sun should successively emit every sort apart. And seeing the sun emits all these sorts at once, they must all together illuminate and paint innumerable equal circles; of all which, being according to their degrees of refrangibility placed in order in a continual series, that oblong spectrum, PT, is composed, which I described in the third Experiment. Now if the sun's circular image γ , [in *fig.* 14, 15] which is made by an unrefracted beam of light, was, by any dilatation of the single rays, or by any other irregularity in the refraction of the first prism, converted into the oblong spectrum, PT: then ought every circle, AG, BH, CJ, &c. in that spectrum, by the cross refraction of the second prism again dilating or otherwise scattering the rays as before, to be in like manner drawn out and transformed into an oblong figure; and thereby the Breadth of the image PT would be now as much augmented, as the Length of the image γ was before by the refraction of the first prism; and thus, by the refraction of both prisms together, would be formed a four-square figure $p\pi l$, as I described above. Wherefore, since the breadth of the spectrum PT is not increased by the refraction sideways, it is certain that the rays are not split or dilated, or otherways irregularly scattered by that refraction, but that every circle is, by a regular and uniform refraction, translated entire into another place, as the circle AG, by the greatest refraction, into the place ag ; the circle BH, by a less refraction, into the place bb ; the circle CJ, by a refraction still less, into the place ci ; and so of the rest: by which means a new spectrum pt , inclined to the former PT, is in like manner composed of circles lying in

in a right line; and these circles must be of the same bigness with the former, because the breadths of all the spectrums, γ , PT and pt , at equal distances from the prisms are equal. A COM-
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I concluded farther, that by the breadth of the hole r , through which the light enters into the dark chamber, there is a penumbra made in the circuit of the spectrum γ , and that penumbra remains in the rectilinear sides of the spectrums PT and pt . I placed therefore at that hole a lens or object-glass of a telescope, which might cast the image of the sun distinctly on γ , without any penumbra at all; and found that the penumbra of the rectilinear sides of the oblong spectrums, PT and pt , was also thereby taken away, so that those sides appeared as distinctly defined, as did the circumference of the first image γ . Thus it happens, if the glass of the prisms be free from veins, and their sides be accurately plane, and well polished without those numberless waves or curls which usually arise from sand-holes a little smoothed with polishing with putty. If the glass be only well polished and free from veins, and the sides not accurately plane, but a little convex or concave, as it frequently happens; yet may the three spectrums, γ , PT and pt , want penumbras, but not in equal distances from the prisms. Now from this want of penumbras, I knew more certainly that every one of the circles was refracted according to some most regular, uniform, and constant law. For if there were any irregularity in the refraction, the right lines AE and GL, which all the circles in the spectrum PT do touch, could not by that refraction be translated into the lines ae and gl , as distinct and straight as they were before; but there would arise, in those translated lines, some penumbra or crookedness or undulation, or other sensible perturbation contrary to what is found by experience. Whatsoever penumbra or perturbation should be made in the circles by the cross refraction of the second prism, all that penumbra or perturbation would be conspicuous in the right lines ae and gl , which touch those circles. And therefore since there is no such penumbra or perturbation in those right lines, there must be none in the circles. Since the distance between those tangents, or breadth of the spectrum, is not increased by the refractions, the diameters of the circles are not

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not increased thereby. Since those tangents continue to be right lines, every circle, which in the first prism is more or less refracted, is exactly in the same proportion more or less refracted in the second. And seeing all these things continue to succeed after the same manner, when the rays are again in a third prism, and again in a fourth, refracted sideways; it is evident, that the rays of one and the same circle, as to their degree of refrangibility, continue always uniform and homogeneous to one another, and that those of several circles do differ in degree of refrangibility, and that in some certain and constant proportion. Which is the thing I was to prove.

There is yet another circumstance or two of this Experiment, by which it becomes still more plain and convincing. Let the second prism DH [in *fig.* 16] be placed not immediately after the first, but at some distance from it; suppose in the mid-way between it and the wall, on which the oblong spectrum pr is cast; so that the light from the first prism may fall upon it in the form of an oblong spectrum $\pi\eta$ parallel to this second prism, and be refracted sideways to form the oblong spectrum pt upon the wall. And you will find as before, that this spectrum pt is inclined to that spectrum pr , which the first prism forms alone without the second; the Blue ends π and p being farther distant from one another, than the Red ones τ and t ; and by consequence that the rays which go to the blue end, π , of the image $\pi\eta$, and which therefore suffer the greatest refraction in the first prism, are again in the second prism more refracted than the rest.

The same thing I tried also by letting the sun's light into a dark room through two little round holes, F and ϕ , [in *fig.* 17] made in the window, and with two parallel prisms, ABC and $a\beta\gamma$, placed at those holes (one at each) refracting those two beams of light to the opposite wall of the chamber; in such manner that the two coloured images, pr and MN , which they there painted, were joined end to end, and lay in one straight line; the Red end τ of the one touching the Blue end m of the other. For if these two refracted beams were again, by a third prism DH placed

cross

(^f) This Experiment may be tried with two prisms only. For the business of the two ABC , $a\beta\gamma$

cross to the two first, refracted sideways, and the spectrums there-<sup>A COM-
POUND.</sup> by translated to some other part of the wall of the chamber, suppose the spectrum pr to pt and the spectrum MN to mn ; these translated spectrums, pt and mn , would not lie in one straight line with their ends contiguous as before, but be broken off from one another and become parallel; the Blue end m of the image mn being, by a greater refraction, translated farther from its former place MT , than the Red end t of the other image pt from the same place MT ; which puts the Proposition past dispute. And this happens whether the third prism, DH , be placed immediately after the two first, or at a great distance from them, so that the light refracted in the two first prisms be either white and circular, or coloured and oblong, when it falls on the third (^f).

Exper. 6. In the middle of two thin boards I made round holes, a third part of an inch in diameter, and in the window-shut a much broader hole being made, to let into my darkened chamber a large beam of the sun's light; I placed a prism behind the shut in that beam, to refract it towards the opposite wall; and close behind the prism I fixed one of the boards, in such manner that the middle of the refracted light might pass through the hole made in it, and the rest be intercepted by the board. Then, at the distance of about twelve feet from the first board, I fixed the other board; in such manner, that the middle of the refracted light, which came through the hole in the first board and fell upon the opposite wall, might pass through the hole in this other board; and the rest, being intercepted by the board, might paint upon it the coloured spectrum of the sun. And close behind this board I fixed another prism, to refract the light, which came through the hole. Then I returned speedily to the first prism; and by turning it slowly to and fro about its axis, I caused the image, which fell upon the second board, to move up and down upon that board, that all its parts might successively pass through the hole in that board, and fall upon the prism behind it. And in the mean time, I noted the places on the opposite wall, to which that light, after its refraction in the second

$a\beta\gamma$ may be performed by a single one having a black paper, with two round holes in it, passed over the side which is turned to the light. Vide *LECT. OPT.* Part II. § 19.

prism,

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prism, did pass; and by the difference of the places I found that the light, which being most refracted in the first prism did go to the Blue end of the image, was again more refracted in the second prism, than the light which went to the Red end of that image; which proves as well the first Proposition as the second. And this happened whether the axis of the two prisms were parallel, or inclined to one another and to the horizon in any given angles.

Illustration. Let F [in *fig. 18*] be the wide hole in the window shut, through which the sun shines upon the first prism ABC, and let the refracted light fall upon the middle of the board DE, and the middle part of that light upon the hole G, made in the middle of that board. Let this trajected part of the light fall again upon the middle of the second board, *de*, and there paint such an oblong coloured image of the sun, as was described in the third Experiment. By turning the prism ABC slowly to and fro about its axis, this image will be made to move up and down the board *de*, and by this means all its parts, from one end to the other, may be made to pass successively through the hole *g*, which is made in the middle of that board. In the mean while another prism, *abc*, is to be fixed next after that hole *g*, to refract the trajected light a second time. And these things being thus ordered, I marked the places, M and N, of the opposite wall upon which the refracted light fell; and found, that whilst the two boards and second prism remained unmoved, those places, by turning the first prism about its axis, were changed perpetually. For when the lower part of the light, which fell upon the second board, *de*, was cast through the hole, *g*, it went to a lower place, M, on the wall; and when the higher part of that light was cast through the same hole, *g*, it went to a higher place, N, on the wall; and when any intermediate part of the light was cast through that hole, it went to some place on the wall between M and N. The unchanged position of the holes in the boards made the incidence of the rays upon the second prism to be the same in all cases. And yet, in that common incidence, some of the rays were more refracted and others less. And those were more refracted in this prism, which, by a greater refraction

in the first prism, were more turned out of the way; and therefore, for their constancy of being more refracted, are deservedly called more refrangible. A COM-
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Exper. 7. At two holes made near one another in my window-shut I placed two prisms, one at each; which might cast upon the opposite wall (after the manner of the third Experiment) two oblong coloured images of the sun. And at a little distance from the wall, I placed a long slender paper with straight and parallel edges, and ordered the prisms and paper so, that the Red colour of one image might fall directly upon one half of the paper, and the Violet colour of the other image upon the other half of the same paper: so that the paper appeared of two colours, Red and Violet, much after the manner of the painted paper in the first and second Experiments. Then with a black cloth I covered the wall behind the paper, that no light might be reflected from it to disturb the Experiment; and viewing the paper through a third prism held parallel to it, I saw that half of it, which was illuminated by the Violet light, to be divided from the other half by a greater refraction, especially when I went a good way off from the paper. For when I viewed it too near at hand, the two halves of the paper did not appear fully divided from one another, but seemed contiguous at one of their angles, like the painted paper in the first Experiment. Which also happened when the paper was too broad.

Sometimes instead of the paper I used a white thread; and this appeared through the prism divided into two parallel threads, as is represented in the nineteenth figure; where DE denotes the thread illuminated with Violet light from D to E, and with Red light from F to G, and *edfg* are the parts of the thread seen by refraction. If one half of the thread be constantly illuminated with Red, and the other half be illuminated with all the colours successively (which may be done by causing one of the prisms to be turned about its axis whilst the other remains unmoved) this other half, in viewing the thread through the prism, will appear in a continued right line with the first half when illuminated with Red; and begin to be a little divided from it, when illuminated with Orange; and remove farther from it, when il-

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Illuminated with Yellow; and still farther when with Green; and farther when with Blue; and go yet farther off, when illuminated with Indigo; and farthest when with deep Violet. Which plainly shews, that the lights of several colours are more and more refrangible from one another, in this order of their colours; Red, Orange, Yellow, Green, Blue, Indigo, deep Violet; and so proves as well the first Proposition as the second.

I caused also the coloured spectrums *PT* [in *fig. 17*] and *MN*, made in a dark chamber by the refractions of two prisms, to lie in a right line end to end, as was described above in the fifth Experiment; and viewing them through a third prism, held parallel to their length, they appeared no longer in a right line, but became broken from one another, as they are represented at *pt* and *mn*; the Violet end, *m*, of the spectrum, *mn*, being by a greater refraction translated farther from its former place, *MT*, than the Red end, *t*, of the other spectrum *pt*.

I farther caused those two spectrums *PT* [in *fig. 20*] and *MN* to become co-incident, in an inverted order of their colours, the Red end of each falling on the Violet end of the other, as they are represented in the oblong figure *PTMN*; and then viewing them through a prism *DH* held parallel to their length, they appeared not co-incident as when viewed with the naked eye, but in the form of two distinct spectrums *pt* and *mn* crossing one another in the middle after the manner of the letter *x*. Which shews, that the Red of the one spectrum and Violet of the other, which were co-incident at *PN* and *MT*, being parted from one another, by a greater refraction of the Violet to *p* and *m*, than of the Red to *n* and *t*, do differ in degrees of refrangibility.

I illuminated also a little circular piece of white paper all over, with the lights of both prisms intermixed; and when it was illuminated with the Red of one spectrum and deep Violet of the other, so as by the mixture of those colours to appear all over Purple, I viewed the paper, first at a less distance, and then at a greater, through a third prism; and as I went from the paper, the refracted image thereof became more and more divided by the unequal refraction of the two mixed colours, and at length parted into two distinct images, a Red one and a Violet one; whereof

whereof the Violet was farthest from the paper, and therefore suffered the greatest refraction. And when the prism at the window, which cast the violet on the paper, was taken away, the Violet image disappeared; but when the other prism was taken away, the Red vanished: which shews, that these two images were nothing else than the lights of the two prisms, which had been intermixed on the purple paper, but were parted again, by their unequal refractions, made in the third prism, through which the paper was viewed. This also was observable, that if one of the prisms at the window, suppose that which cast the Violet on the paper, was turned about its axis to make all the colours in this order, Violet, Indigo, Blue, Green, Yellow, Orange, Red, fall successively on the paper from that prism, the Violet image changed colour accordingly, turning successively to Indigo, Blue, Green, Yellow and Red; and in changing colour came nearer and nearer to the Red image made by the other prism, until, when it was also Red, both images became fully co-incident.

I placed also two paper-circles very near one another, the one in the Red light of one prism, and the other in the Violet light of the other. The circles were each of them an inch in diameter, and behind them the wall was dark, that the Experiment might not be disturbed by any light coming from thence. These circles thus illuminated, I viewed through a prism, so held that the refraction might be made towards the Red circle, and as I went from them they came nearer and nearer together, and at length became co-incident; and afterwards when I went still farther off, they parted again in a contrary order, the Violet by a greater refraction being carried beyond the Red.

Exper. 8. In Summer, when the sun's light uses to be strongest, I placed a prism at the hole of the window-shut, as in the third Experiment, yet so that its axis might be parallel to the axis of the World, and at the opposite wall, in the sun's refracted light, I placed an open book. Then going six feet and two inches from the book, I placed there the above-mentioned lens, by which the light, reflected from the book, might be made to converge and meet again at the distance of six feet and two inches

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behind the lens, and there paint the species of the book upon a sheet of white paper, much after the manner of the second Experiment. The book and lens being made fast, I noted the place where the paper was, when the letters of the book, illuminated by the fullest Red light of the solar image falling upon it, did cast their species on that paper most distinctly: and then I stayed, till by the motion of the sun, and consequent motion of his image on the book, all the colours from that Red to the middle of the Blue passed over those letters; and when those letters were illuminated by that Blue, I noted again the place of the paper, when they cast their species most distinctly upon it: and I found that this last place of the paper was nearer to the lens, than its former place, by about two inches and an half, or two and three quarters. So much sooner therefore did the light in the Violet end of the image, by a greater refraction, converge and meet, than the light in the Red end. But in trying this the chamber was as dark as I could make it. For if these colours be diluted and weakened by the mixture of any adventitious light, the distance between the places of the paper will not be so great. This distance, in the second Experiment, where the colours of Natural Bodies were made use of, was but an inch and an half, by reason of the imperfection of those colours. Here, in the colours of the Prism, which are manifestly more full, intense, and lively than those of Natural Bodies, the distance is two inches and three quarters. And were the colours still more full, I question not but that the distance would be considerably greater. For the coloured light of the prism, by the interfering of the circles described in the second figure of the fifth Experiment, and also by the light of the very bright clouds next the sun's body intermixing with these colours, and by the light scattered by the inequalities in the polish of the prism, was so very much compounded, that the species, which those faint and dark colours, the Indigo and Violet, cast upon the paper, were not distinct enough to be well observed.

Exper. 9. A prism, whose two angles at its base were equal to one another and half right ones, and the third a right one, I placed in a beam of the sun's light let into a dark chamber,

through a hole in the window-shut, as in the third Experiment. And turning the prism slowly about its axis, until all the light which went through one of its angles, and was refracted by it, began to be reflected by its base, at which till then it went out of the glass; I observed that those rays, which had suffered the greatest refraction, were sooner reflected than the rest. I conceived therefore that those rays of the Reflected light, which were most Refrangible, did first of all, by a total reflexion, become more copious in that light than the rest; and that afterwards the rest also, by a total reflexion, became as copious as these. To try this, I made the Reflected light pass through another prism, and being refracted by it to fall afterwards upon a sheet of white paper placed at some distance behind it; and there, by that Refraction, to paint the usual colours of the prism. And then causing the first prism to be turned about its axis as above, I observed that when those rays, which in this prism had suffered the greatest refraction, and appeared of a Blue and Violet colour, began to be totally reflected, the Blue and Violet light on the paper, which was most refracted in the second prism, received a sensible increase above that of the Red and Yellow, which was least refracted; and afterwards when the rest of the light, which was Green, Yellow and Red, began to be totally reflected in the first prism, the light of those colours on the paper received as great an increase, as the Violet and Blue had done before. Whence it is manifest, that the beam of light reflected by the base of the prism, being augmented first by the more refrangible rays, and afterwards by the less refrangible ones, is compounded of rays differently refrangible. And that all such Reflected light is of the same nature with the sun's light, before its incidence on the base of the prism, no man ever doubted: it being generally allowed, that light, by such reflexions, suffers no alteration in its modifications and properties. I do not here take notice of any refractions made in the sides of the first prism, because the light enters it perpendicularly at the first side, and goes out perpendicularly at the second side, and therefore suffers none. So then, the sun's incident light being of the same temper and constitution with his emergent light, and the last being compounded of rays differently

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LIGHT differently refrangible, the first must be in like manner compounded.

Illustration. In the twenty-first figure, ABC is the first prism; BC its base; B and C its equal angles at the base, each of 45 degrees; A its rectangular vertex; FM a beam of the sun's light, let into a dark room through a hole, F, one third part of an inch broad; M its incidence on the base of the prism; MG a less refracted ray; MH a more refracted ray; MN the beam of light reflected from the base; VXY the second prism, by which this beam, in passing through it, is refracted; Nt the less refracted light of this beam; and Np the more refracted part thereof. When the first prism, ABC, is turned about its axis, according to the order of the letters ABC, the rays MH emerge more and more obliquely out of that prism, and at length after their most oblique emergence are reflected towards N, and going on to p do increase the number of the rays Np. Afterwards by continuing the motion of the first prism, the rays MG are also reflected to N, and increase the number of the rays Nt. And therefore the light MN admits into its composition, first the more refrangible rays, and then the less refrangible rays; and yet, after this composition, is of the same nature with the sun's immediate light FM, the reflexion of the specular base BC causing no alteration therein (S).

Exper. 10. Two prisms, which were alike in shape, I tied so together, that their axes and opposite sides being parallel, they composed a parallelepiped. And, the sun shining into my dark chamber through a little hole in the window-shut, I placed that parallelepiped in his beam at some distance from the hole, in such a posture, that the axes of the prisms might be perpendicular to the incident rays; and that those rays, being incident upon the first side of one prism, might go on through the two contiguous sides of both prisms, and emerge out of the last side of the second prism. This side, being parallel to the first side of the first prism, caused the emerging light to be parallel to the incident. Then, beyond these two prisms I placed a third, which might refract that emergent light, and by that refraction cast

(S) Vid. Lect. Opt. Part II. § 71—75.

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the usual colours of the prism upon the opposite wall, or upon a sheet of white paper, held at a convenient distance behind the prism, for that refracted light to fall upon it. After this I turned the parallelepiped about its axis; and found, that when the contiguous sides of the two prisms became so oblique to the incident rays, that those rays began all of them to be reflected, those rays, which in the third prism had suffered the greatest Refraction and painted the paper with Violet and Blue, were first of all by a total Reflexion taken out of the transmitted light; the rest remaining, and on the paper painting their colours of Green, Yellow, Orange, and Red, as before; and afterwards by continuing the motion of the two prisms, the rest of the rays also, by a total Reflexion, vanished in order, according to their degrees of Refrangibility. The light therefore, which emerged out of the two prisms, is compounded of rays differently refrangible; seeing the more refrangible rays may be taken out of it; while the less refrangible remain. But this light, being trajected only through the parallel superficies of the two prisms, if it suffered any change by the refraction of one superficies, it lost that impression by the contrary refraction of the other superficies; and so, being restored to its pristine constitution, became of the same nature and condition as at first, before its incidence on those prisms; and therefore, before its incidence, was as much compounded of rays differently refrangible, as afterwards.

Illustration. In the twenty-second figure, ABC and BCD are the two prisms tied together in the form of a parallelepiped, their sides BC and CB being contiguous, and their sides AB and CD parallel. And HJK is the third prism, by which the sun's light, propagated through the hole F into the dark chamber, and there passing through those sides of the prisms, AB, BC, CB and CD, is refracted at O to the white paper PT; falling there partly upon P by a greater refraction, partly upon T by a less refraction, and partly upon R, and other intermediate places, by intermediate refractions. By turning the parallelepiped ACBD about its axis, according to the order of the letters A, C, D, B, at length when the contiguous planes, BC and CB, become sufficiently oblique to the rays FM, which are incident upon them at M, there will vanish

nish totally out of the refracted light opt , first of all the most refracted rays op , (the rest or and ot remaining as before) then the rays or , and other intermediate ones, and lastly, the least refracted rays ot . For when the plane bc becomes sufficiently oblique to the rays incident upon it, those rays will begin to be totally reflected by it towards N ; and first the most refrangible rays will be totally reflected (as was explained in the preceding Experiment) and by consequence must first disappear at P ; and afterwards the rest, as they are in order totally reflected to N , they must appear in the same order at R and T . So then the rays, which at o suffer the greatest refraction, may be taken out of the light mo , whilst the rest of the rays remain in it; and therefore that light mo is compounded of rays differently refrangible. And because the planes ab and cd are parallel, and therefore by equal and contrary refractions destroy one another's effects, the incident light fm must be of the same kind and nature with the emergent light mo , and therefore doth also consist of rays differently refrangible. These two lights, fm and mo , before the most refrangible rays are separated out of the emergent light mo , agree in colour, and in all other properties so far as my observation reaches; and therefore are deservedly reputed of the same nature and constitution, and by consequence the one is compounded as well as the other. But after the most refrangible rays begin to be totally reflected, and thereby separated out of the emergent light mo , that light changes its colour from White to a dilute and faint Yellow, a pretty good Orange, a very full Red successively, and then totally vanishes. For after the most refrangible rays, which paint the paper at P with a Purple colour, are, by a total reflexion, taken out of the beam of light mo ; the rest of the colours, which appear on the paper at R and T , being mixed in the light mo , compound there a faint Yellow: and after the Blue and part of the Green which appear on the paper between P and R are taken away; the rest, which appear between R and T , that is the Yellow, Orange, Red, and a little Green, being mixed in the beam mo compound there an Orange; and when all the rays are by reflexion taken out of the beam mo , except the least refrangible, which at T appear of a full Red, their

their colour is the same in that beam mo , as afterwards at T , the refraction of the prism hjk serving only to separate the differently refrangible rays, without making any alteration in their colours, as shall be more fully proved hereafter. All which confirms as well the first Proposition as the second.

Scholium. If this Experiment and the former be conjoined and made one, by applying a fourth prism vxy [in *fig. 22*] to refract the reflected beam mn towards pt , the conclusion will be clearer. For then the light np , which in the fourth prism is more refracted, will become fuller and stronger when the light op , which in the third prism hjk is more refracted, vanishes at P ; and afterwards, when the less refracted light, or , vanishes at T , the less refracted light nt will become encreased, whilst the more refracted light at p receives no farther encrease. And as the trajected beam, mo , in vanishing is always of such a colour, as ought to result from the mixture of the colours which fall upon the paper PT , so is the reflected beam, mn , always of such a colour, as ought to result from the mixture of the colours which fall upon the paper pt . For when the most refrangible rays are, by a total reflexion, taken out of the beam mo , and leave that beam of an Orange colour, the excess of those rays in the reflected light, does not only make the Violet, Indigo and Blue at p more full, but also makes the beam mn change from the yellowish colour of the sun's light, to a pale White inclining to Blue, and afterward recover its yellowish colour again, so soon as all the rest of the transmitted light mor is reflected.

Now seeing that in all this variety of Experiments, whether the trial be made in light Reflected, and that either from Natural Bodies, as in the first and second Experiment, or Specular, as in the ninth; or in light Refracted, and that, either before the unequally refracted rays are, by diverging, separated from one another, and losing their whiteness which they have altogether, appear severally of several colours, as in the fifth Experiment; or after they are separated from one another, and appear coloured as in the sixth, seventh, and eighth Experiments; or in light trajected through parallel superficies, destroying each others effects, as in the tenth Experiment; there are always found rays,

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which at equal incidences on the same Medium suffer unequal refractions; and that without any splitting or dilating of single rays, or contingency in the inequality of the refractions, as is proved in the fifth and sixth Experiments: and seeing the rays which differ in refrangibility may be parted and sorted from one another; and that either by Refraction, as in the third Experiment, or by Reflexion, as in the tenth; and then the several sorts apart at equal incidences suffer unequal refractions, and those sorts are more refracted than others after separation, which were more refracted before it, as in the sixth and following Experiments; and if the Sun's light be trajected through three or more cross prisms successively, those rays, which in the first prism are refracted more than others, are in all the following prisms refracted more than others in the same rate and proportion, as appears by the fifth Experiment: it is manifest, that the Sun's Light is an Heterogeneous Mixture of rays, some of which are constantly more Refrangible than others, as was proposed.

PROP. III. THEOR. III.

The Sun's light consists of rays differing in Reflexibility, and those rays are more reflexible than others, which are more refrangible.

This is manifest by the ninth and tenth Experiments: for in the ninth Experiment, by turning the prism about its axis, until the rays within it, which in going out into the air were refracted by its base, became so oblique to the base, as to begin to be totally reflected thereby; those rays became first of all totally reflected, which before, at equal incidences with the rest, had suffered the greatest refraction. And the same thing happens in the reflexion made by the common base of the two prisms in the tenth Experiment.

PROP. IV. PROB. I.

To separate from one another the Heterogeneous rays of Compound Light.

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The heterogeneous rays are in some measure separated from one another by the refraction of the prism in the third Experiment;

ment; and in the fifth Experiment, by taking away the penumbra from the rectilinear sides of the coloured image, that separation in those very rectilinear sides or straight edges of the image becomes perfect. But in all places between those rectilinear edges, those innumerable circles there described, which are severally illuminated by homogeneous rays, by interfering with one another, and being every where commixed, do render the light sufficiently compound. But if these circles, whilst their centers keep their distances and positions, could be made less in diameter, their interfering one with another, and by consequence the mixture of the heterogeneous rays would be proportionally diminished. In the twenty-third figure let AG, BH, CJ, DK, EL, FM be the circles, which so many sorts of rays, flowing from the same disque of the sun, do in the third Experiment illuminate; of all which and innumerable other intermediate ones, lying in a continual series between the two rectilinear and parallel edges of the sun's oblong image PT , that image is composed; as was explained in the fifth Experiment. And let ag, bh, ci, dk, el, fm , be so many less circles, lying in a like continual series between two parallel right lines af and gm , with the same distances between their centers, and illuminated by the same sorts of rays; that is, the circle ag with the same sort by which the corresponding circle AG was illuminated; and the circle bh with the same sort by which the corresponding circle BH was illuminated; and the rest of the circles, ci, dk, el, fm , respectively, with the same sorts of rays by which the several corresponding circles, CJ, DK, EL, FM , were illuminated. In the figure, PT , composed of the greater circles, three of those circles, AG, BH, CJ , are so expanded into one another, that the three sorts of rays by which those circles are illuminated, together with other innumerable sorts of intermediate rays, are mixed at QR in the middle of the circle BH . And the like mixture happens throughout almost the whole length of the figure PT . But in the figure, pt , composed of the less circles, the three less circles, ag, bh, ci , which answer to those three greater, do not extend into one another; nor are there any where mingled so much as any two of the three sorts of rays,

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Now he that shall thus consider it, will easily understand, that the mixture is diminished in the same proportion with the diameters of the circles. If the diameters of the circles, whilst their centers remain the same, be made three times less than before, the mixture will be also three times less; if ten times less, the mixture will be ten times less, and so of other proportions. That is, the mixture of the rays in the greater figure, PT, will be to their mixture in the less, pt, as the latitude of the greater figure is to the latitude of the less. For the latitudes of these figures are equal to the diameters of their circles. And hence it easily follows, that the mixture of the rays in the refracted spectrum, pt, is to the mixture of the rays in the direct and immediate light of the sun, as the breadth of that spectrum is to the difference between the length and breadth of the same spectrum.

So then, if we would diminish the mixture of the rays, we are to diminish the diameters of the circles. Now these would be diminished if the sun's diameter to which they answer could be made less than it is, or (which comes to the same purpose) if without doors, at a great distance from the prism towards the sun, some opaque body were placed, with a round hole in the middle of it, to intercept all the sun's light, excepting so much as, coming from the middle of his body, could pass through that hole to the prism. For so the circles AG, BH, and the rest, would not any longer answer to the whole disk of the sun, but only to that part of it, which could be seen from the prism through that hole, that it is to the apparent magnitude of that hole viewed from the prism. But that these circles may answer more distinctly to that hole, a lens is to be placed by the prism to cast the image of the hole, (that is, every one of the circles AG, BH, &c.) distinctly upon the paper at PT; after such a manner as by a lens, placed at a window, the species of objects abroad are cast distinctly upon a paper within the room, and the rectilinear sides of the oblong solar image in the fifth Experiment became distinct without any penumbra. If this be done, it will not be necessary to place that hole very far off, no not beyond the window. And therefore

therefore instead of that hole, I used the hole in the window-shut as follows. into its constituent Parts.

Exper. 11. In the sun's light let into my darkened chamber, through a small round hole in my window-shut, at about ten or twelve feet from the window, I placed a lens, by which the image of the hole might be distinctly cast upon a sheet of white paper, placed at the distance of six, eight, ten or twelve feet from the lens. For according to the difference of the lenses I used various distances, which I think not worth the while to describe. Then immediately after the lens I placed a prism, by which the projected light might be refracted either upwards or sideways, and thereby the round image, which the lens alone did cast upon the paper, might be drawn out into a long one with parallel sides, as in the third Experiment. This oblong image I let fall upon another paper, at about the same distance from the prism as before, moving the paper either towards the prism or from it, until I found the just distance where the rectilinear sides of the image became most distinct. For in this case, the circular images of the hole which compose that image, after the same manner that the circles *ag, bb, ci, &c.* do the figure *pt* [in *fig. 23*] were terminated most distinctly without any penumbra, and therefore extended into one another the least that they could, and by consequence the mixture of the heterogeneous rays was now the least of all. By this means I used to form an oblong image (such as is *pt*) [in *fig. 23* and *24*] of circular images of the hole (such as are *ag, bb, ci, &c.*) and by using a greater or less hole in the window-shut, I made the circular images *ag, bb, ci, &c.* of which it was formed, to become greater or less at pleasure, and thereby the mixture of the rays in the image *pt* to be as much or as little as I desired.

Illustration. In the twenty-fourth figure, F represents the circular hole in the window-shut; MN the lens whereby the image or species of that hole is cast distinctly upon a paper at J; ABC the prism, whereby the rays are, at their emerging out of the lens, refracted from J towards another paper at *pt*, and the round image at J is turned into an oblong image, *pt*, falling on that other paper. This image, *pt*, consists of circles placed one after

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another in a rectilinear order, as was sufficiently explained in the fifth Experiment; and these circles are equal to the circle *J*, and consequently answer in magnitude to the hole *F*; and therefore by diminishing that hole they may be at pleasure diminished, whilst their centers remain in their places. By this means I made the breadth of the image *pt* to be forty times, and sometimes sixty or seven times less than its length. As for instance, if the breadth of the hole *F* be one tenth of an inch, and *MF* the distance of the lens from the hole be 12 feet; and if *pB* or *pM* the distance of the image *pt* from the prism or lens be 10 feet, and the Refracting angle of the prism be 62 degrees, the breadth of the image *pt* will be one twelfth of an inch, and the length about six inches; and therefore the length to the breadth as 72 to 1, and by consequence the light of this image 71 times less compound than the sun's direct light. And light thus far simple and homogeneous, is sufficient for trying all the Experiments in this Book about Simple Light. For the composition of heterogeneous rays is in this light so little, that it is scarce to be discovered and perceived by sense, except perhaps in the Indigo and Violet. For these, being dark colours, do easily suffer a sensible alloy by that little scattering light, which uses to be refracted irregularly by the inequalities of the prism.

Yet instead of the circular hole *F*, it is better to substitute an oblong hole, shaped like a long parallelogram with its length parallel to the prism *ABC*. For if this hole be an inch or two long, and but a tenth or twentieth part of an inch broad, or narrower: the light of the image *pt* will be as simple as before, or simpler, and the image will become much broader, and therefore more fit to have experiments tried in its light than before.

Instead of this parallelogram hole may be substituted a triangular one of equal sides, whose base for instance is about the tenth part of an inch, and its height an inch or more. For by this means, if the axis of the prism be parallel to the perpendicular of the triangle, the image *pt* [in *fig.* 25] will now be formed of equicrural triangles *ag*, *bb*, *ci*, *dk*, *el*, *fm*, &c. and innumerable other intermediate ones answering to the triangular hole in shape and bigness, and lying one after another in a continual series

ries between two parallel lines *af* and *gm*. These triangles are a little intermingled at their bases, but not at their vertices; and therefore the light on the brighter side *af* of the image, where the bases of the triangles are, is a little compounded, but on the darker side *gm* is altogether uncompounded, and in all places between the sides the composition is proportional to the distances of the places from that obscurer side *gm*. And having a spectrum *pt* of such a composition, we may try experiments either in its stronger and less simple light near the side *af*, or in its weaker and simple light near the other side *gm*, as it shall seem most convenient.

But in making experiments of this kind the chamber ought to be made as dark as can be, lest any foreign light mingle itself with the light of the spectrum *pt*, and render it compound; especially if we would try experiments in the more simple light next the side *gm* of the spectrum; which being fainter, will have a less proportion to the foreign light, and so, by the mixture of that light, be more troubled and made more compound. The lens also ought to be good, such as may serve for Optical uses; and the prism ought to have a large angle, suppose of 65 or 70 degrees, and to be well wrought, being made of glass free from bubbles and veins, with its sides not a little convex or concave, as usually happens, but truly plane, and its polish elaborate, as in working Optick-glasses, and not such as is usually wrought with putty; whereby the edges of the sand-holes being worn away, there are left all over the glass a numberless company of very little convex polite risings, like waves. The edges also of the prism and lens, so far as they may make any irregular refraction, must be covered with a black paper glewed on. And all the light of the sun's beam let into the chamber, which is useless and unprofitable to the Experiment, ought to be intercepted with black paper, or other black obstacles. For otherwise the useless light, being reflected every way in the chamber, will mix with the oblong spectrum, and help to disturb it. In trying these things so much diligence is not altogether necessary, but it will promote the success of the Experiments, and by a very scrupulous examiner of things deserves to be applied. It is difficult to

to get glass-prisms fit for this purpose, and therefore I used sometimes Prismatick Vessels, made with pieces of broken looking-glasses, and filled with rain-water. And to increase the refraction, I sometimes impregnated the water strongly with *Saccharum Saturni*.

P R O P. V. T H E O R. IV.

Homogeneous Light is refracted regularly, without any dilatation, splitting, or shattering of the rays; and the confused Vision of objects, seen through refracting bodies by heterogeneous light, arises from the different Refrangibility of several sorts of rays.

Regular Refraction of Homogeneous Light.

The first part of this Proposition has been already sufficiently proved in the fifth Experiment, and will farther appear by the Experiments which follow.

Exper. 12. In the middle of a black paper I made a round hole, about a fifth or sixth part of an inch in diameter. Upon this paper I caused the spectrum of Homogeneous Light, described in the former Proposition, so to fall, that some part of the light might pass through the hole of the paper. This transmitted part of the light I refracted with a prism placed behind the paper, and letting this refracted light fall perpendicularly upon a white paper, two or three feet distant from the prism, I found that the spectrum formed on the paper by this light was not oblong, as when it is made (in the third Experiment) by refracting the sun's compound light, but was (so far as I could judge by my eye) perfectly circular, the length being no greater than the breadth. Which shews that this light is refracted regularly without any dilatation of the rays.

Exper. 13. In the Homogeneous Light I placed a paper-circle of a quarter of an inch in diameter, and in the sun's unrefracted Heterogeneous White Light I placed another paper-circle of the same bigness. And going from the papers to the distance of some feet, I viewed both circles through a prism. The circle illuminated by the sun's Heterogeneous Light appeared very oblong, as in the fourth Experiment, the length being many times greater than the breadth; but the other circle illuminated with Homogeneous

geneal Light appeared circular and distinctly defined, as when it is viewed with the naked eye. Which proves the whole Proposition.

Exper. 14. In the Homogeneous Light I placed flies, and such like minute objects, and viewing them through a prism, I saw their parts as distinctly defined, as if I had viewed them with the naked eye. The same objects placed in the sun's unrefracted Heterogeneous Light, which was white, I viewed also through a prism, and saw them most confusedly defined, so that I could not distinguish their smaller parts from one another. I placed also the letters of a small print one while in the Homogeneous Light, and then in the Heterogeneous; and viewing them through a prism, they appeared, in the latter case, so confused and indistinct that I could not read them; but in the former, they appeared so distinct that I could read readily, and thought I saw them as distinct, as when I viewed them with my naked eye. In both cases I viewed the same objects, through the same prism, at the same distance from me, and in the same situation. There was no difference but in the light, by which the objects were illuminated, and which in one case was simple, and in the other compound; and therefore the distinct vision in the former case, and confused in the latter, could arise from nothing else than from that difference of the lights. Which proves the whole Proposition.

And in these three Experiments it is farther very remarkable, that the colour of Homogeneous Light was never changed by the refraction.

P R O P. VI. T H E O R. V.

The sine of Incidence of every ray considered apart, is to its sine of Refraction in a given ratio.

That every Ray, considered apart, is constant to itself in some degree of refrangibility, is sufficiently manifest out of what has been said. Those rays which in the first refraction are, at equal incidences, most refracted, are also in the following refractions, at equal incidences, most refracted; and so of the least refrangible, and the rest which have any mean degree of refrangibility.

Given proportions between the

ty, as is manifest by the fifth, sixth, seventh, and eighth, and ninth Experiments. And those which the first time, at like incidences, are equally refracted, are again at like incidences equally and uniformly refracted; and that, whether they be refracted before they be separated from one another, as in the fifth Experiment; or whether they be refracted apart, as in the twelfth, thirteenth and fourteenth Experiments. The refraction therefore of every ray apart is regular, and what rule that refraction observes we are now to shew.

The late Writers in Opticks teach, that the sines of incidence are in a given proportion to the sines of refraction, as was explained in the fifth Axion: and some, by instruments fitted for measuring of refractions, or otherwise experimentally examining this proportion, do acquaint us that they have found it accurate. But whilst they, not understanding the different refrangibility of several rays, conceived them all to be refracted according to one and the same proportion, it is to be presumed that they adapted their measures only to the middle of the refracted light; so that from their measures we may conclude only, that the rays which have a mean degree of refrangibility, that is those which, when separated from the rest, appear Green, are refracted according to a given proportion of their sines. And therefore we are now to shew that the like given proportions obtain in all the rest. That it should be so is very reasonable, Nature being ever conformable to herself: but an experimental proof is desired. And such a proof will be had, if we can shew that the sines of refraction of rays differently refrangible are one to another in a given proportion, when their sines of incidence are equal. For if the sines of refraction of all the rays are in given proportions to the sine of refraction of a ray which has a mean degree of refrangibility, and this sine is in a given proportion to the equal sines of incidence, those other sines of refraction will also be in given proportions to the equal sines of incidence. Now when the sines of incidence are equal; it will appear by the following Experiment that the sines of refraction are in a given proportion to one another.

Exper.

Exper. 15. The sun shining into a dark chamber through a little round hole in the window-shut, let *s* [in *fig.* 26.] represent his round white image painted on the opposite wall by his direct light; *pr*, his oblong coloured image made by refracting that light with a prism placed at the window; and *pt*, or *2p2t*, or *3p3t*, his oblong coloured image made by refracting again the same light sideways, with a second prism placed immediately after the first in a cross position to it, as was explained in the fifth Experiment: that is to say, *pt* when the refraction of the second prism is small, *2p2t* when its refraction is greater, and *3p3t* when it is greatest. For such will be the diversity of the refractions if the Refracting Angle of the second prism be of various magnitudes; suppose of fifteen or twenty degrees, to make the image *pt*; of thirty or forty, to make the image *2p2t*; and of sixty, to make the image *3p3t*. But for want of solid glass-prisms with angles of convenient bignesses, there may be vessels made of polished plates of glass, cemented together in the form of prisms, and filled with water. These things being thus ordered, I observed that all the solar images or coloured spectrums *pr*, *pt*, *2p2t*, *3p3t* did very nearly converge to the place, *s*, on which the direct light of the sun fell, and painted his white round image, when the prisms were taken away. The axis of the spectrum *pr*, that is the line drawn through the middle of it parallel to its rectilinear sides, did, when produced, pass exactly through the middle of that white round image *s*. And when the refraction of the second prism was equal to the refraction of the first, the Refracting Angle of them both being about 60 degrees, the axis of the spectrum *3p3t* made by that refraction, did, when produced, pass also through the middle of the same white round image *s*. But when the refraction of the second prism was less than that of the first, the produced axes of the spectrums, *tp*, or *2t2p*, made by that refraction did cut the produced axis of the spectrum *pr* in the points *m* and *n*, a little beyond the center of that white round Image *s*. Whence the proportion of the line *3tr* to the line *3pr* was a little greater than the proportion of *2tr* to *2pr*, and this proportion a little greater than that of *tr* to *pr*. Now when the light of the spectrum *pr* falls perpendicularly

H 2

upon

Given proportions between the

upon the wall, those lines $3tT$, $3pP$, and $2tT$, $2pP$ and tT , pP , are the tangents of the refractions; and therefore by this Experiment the proportions of the tangents of the refractions are obtained, from whence the proportions of the sines being derived, they come out equal, so far as by viewing the spectrums, and using some mathematical reasoning, I could estimate. For I did not make an accurate computation. So then the Proposition holds true in every ray apart, so far as appears by Experiment. And that it is accurately true, may be demonstrated upon this supposition. *That bodies refract light by acting upon its rays in lines perpendicular to their surfaces.* But in order to this demonstration, I must distinguish the motion of every ray into two motions, the one perpendicular to the refracting surface, the other parallel to it, and concerning the perpendicular motion lay down the following Proposition.

If any motion or moving thing whatsoever be incident, with any velocity, on any broad and thin space, terminated on both sides by two parallel planes; and, in its passage through that space, be urged perpendicularly towards the farther plane, by any force which at given distances from the plane is of given quantities; the perpendicular velocity of that motion or thing, at its emerging out of that space, shall be always equal to the square root of the sum of the square of the perpendicular velocity of that motion or thing, at its incidence on that space, and of the square of the perpendicular velocity which that motion or thing would have at its emergence, if, at its incidence, its perpendicular velocity was infinitely little.

And the same Proposition holds true of any motion or thing perpendicularly retarded in its passage through that space, if instead of the sum of the two squares you take their difference. The demonstration Mathematicians will easily find out, and therefore I shall not trouble the reader with it (g).

Suppose now that a ray coming most obliquely in the line MC [in

(g) See the figure of the xxvth Proposition of the first book of the Principia. In that figure AN and Id are equal. Also TH and IK . GH and IK are as the whole velocities of the incident and emergent ray. Therefore GA and Kd are as the perpendicular velocities of incidence and emergence. But $Kd = \sqrt{IK^2 - Id^2} = \sqrt{TH^2 - AN^2} = \sqrt{TH^2 - GH^2 + GA^2}$. Hence if GA ,

[in *fig. 1.*] be refracted at c by the plane RS into the line CN ; and if it be required to find the line CE , into which any other ray AC shall be refracted, let MC , AD , be the sines of incidence of the two rays, and NG , EF , their sines of refraction; and let the equal motions of the incident rays be represented by the equal lines MC and AC ; and the motion MC being considered as parallel to the refracting plane, let the other motion AC be distinguished into two motions AD and DC ; one of which AD is parallel, and the other DC perpendicular to the refracting surface. In like manner, let the motions of the emerging rays be distinguished into two; whereof the perpendicular ones are $\frac{MC}{NG} CG$ and $\frac{AD}{EF} CF$. And if the force of the refracting plane begins to act upon the rays either in that plane, or at a certain distance from it on the one side, and ends at a certain distance from it on the other side, and in all places between those two limits acts upon the rays in lines perpendicular to that refracting plane, and the actions upon the rays at equal distances from the refracting plane be equal, and at unequal ones either equal or unequal according to any rate whatever: that motion of the ray, which is parallel to the refracting plane, will suffer no alteration by that force; and that motion, which is perpendicular to it, will be altered according to the rule of the foregoing Proposition. If therefore for the perpendicular velocity of the emerging ray, CN , you write $\frac{MC}{NG} CG$, as above; then the perpendicular velocity of any other emerging ray, CE , which was $\frac{AD}{EF} CF$, will be equal to the square root of $CDq + \frac{MCq}{NGq} CGq$. And by squaring these equals, and adding to them the equals ADq and $MCq - CDq$, and dividing the sums by the equals $CFq + EFq$, and $CGq + NGq$, you will have $\frac{ADq}{EFq}$ equal to $\frac{MCq}{NGq}$. Whence AD , the sine of incidence, is to EF the sine of refraction, as MC to NG , that is, in a given ratio. And this demonstration being general, without determining what light is, or by what kind of force it is refracted, or assuming any thing farther than that the refracting

GA , the perpendicular velocity of incidence, be gradually diminished till it be reduced to nothing; Kd , the perpendicular velocity of emergence, becomes ultimately equal to $\sqrt{TH^2 - GH^2}$. And in any given magnitude of GA , Kd is equal to the square root of the sum of the square of that its ultimate quantity and the square of GA . And this is what Newton here affirms.

body acts upon the rays in lines perpendicular to its surface; I take it to be a very convincing argument of the full truth of this Proposition^(h).

So then, if the *ratio* of the sines of incidence and refraction of any sort of rays be found in any one case, it is given in all cases; and this may be readily found by the method in the following Proposition.

P R O P. VII. T H E O R. VI.

The perfection of Telescopes is impeded by the different refrangibility of the rays of light.

The imperfection of Telescopes is vulgarly attributed to the Spherical Figures of the glasses, and therefore Mathematicians have propounded to figure them by the Conical Sections. To shew that they are mistaken, I have inserted this Proposition; the truth of which will appear by the measures of the refractions of the several sorts of rays; and these measures I thus determine.

To measure
the different
Refractions of
the extreme
Rays.

In the third Experiment of this first Part, where the Refracting Angle of the prism was $62\frac{1}{2}$ degrees, the half of that angle 31 deg. 15 min. is the angle of incidence of the rays, at their going out of the glass into the air⁽ⁱ⁾; and the sine of this angle is 5188 , the radius being 10000 . When the axis of this prism was parallel to the horizon, and the refraction of the rays, at their incidence on this prism, equal to that at their emergence out of it, I observed with a quadrant the angle, which the mean refrangible rays (that is, those which went to the middle of the sun's coloured image) made with the horizon; and by this angle and the sun's altitude observed at the same time, I found the angle which the emergent rays contained with the incident to be 44 deg. and 40 min. and the half of this angle added to the angle of incidence 31 deg. 15 min. makes the angle of refraction^(k), which is therefore 53 deg. 35 min. and its sine 8047 . These are the sines

^(h) This Proposition is the xxvth of the first book of the Principia, where it is more elegantly proved.

⁽ⁱ⁾ Vide Lect. Opt. Part I. § 31. Lem. I.

of

of incidence and refraction of the mean refrangible rays, and their proportion in round numbers is 20 to 31 . This glass was of a colour inclining to green. The last of the prisms mentioned in the third Experiment was of clear white glass. Its Refracting Angle $63\frac{1}{2}$ degrees. The angle, which the emergent rays contained with the incident, 45 deg. 50 min. The sine of half the first angle 5262 . The sine of half the sum of the angles 8157 . And their proportion in round numbers 20 to 31 , as before.

To measure
the different
Refractions of
the extreme
Rays.

From the length of the image, which was about $9\frac{3}{4}$ or 10 inches, subduct its breadth, which was $2\frac{1}{8}$ inches, and the remainder $7\frac{3}{4}$ inches would be the length of the image, were the sun but a point; and therefore subtends the angle which the most and least refrangible rays, when incident on the prism in the same lines, do contain with one another after their emergence. Whence this angle is 2 deg. $0'$. $7''$. For the distance between the image and the prism, where this angle is made, was $18\frac{1}{2}$ feet, and at that distance the chord, $7\frac{3}{4}$ inches, subtends an angle of 2 deg. $0'$. $7''$. Now half this angle is the angle which these emergent rays contain with the emergent mean refrangible rays, and a quarter thereof, that is $30'$. $2''$, may be accounted the angle which they would contain with the same emergent mean refrangible rays, were they co-incident to them within the glass, and suffered no other refraction than that at their emergence. For if two equal refractions, the one at the incidence of the rays on the prism, the other at their emergence, make half the angle 2 deg. $0'$. $17''$; then one of those refractions will make about a quarter of that angle; and this quarter added to and subducted from the angle of refraction of the mean refrangible rays, which was 53 deg. $35'$, gives the angles of refraction of the most and least refrangible rays 54 deg. $5'$. $2''$, and 53 deg. $4'$. $58''$, whose sines are 8099 and 7995 , the common angle of incidence being 31 deg. $15'$, and its sine 5188 ; and these sines, in the least round numbers, are in proportion to one another, as 78 and 77 to 50 ^(l).

^(k) Lect. Opt. Part I. § 31. Lem. II.

^(l) Lect. Opt. Part I. § 32.

Now

Now if you subduct the common sine of incidence, 50, from the sines of refraction, 77 and 78; the remainders, 27 and 28, shew that in small refractions the refraction of the least refrangible rays is to the refraction of the most refrangible ones as 27 to 28 very nearly, and that the difference of the refractions of the least refrangible and most refrangible rays is about the $27\frac{1}{2}$ th part of the whole refraction of the mean refrangible rays.

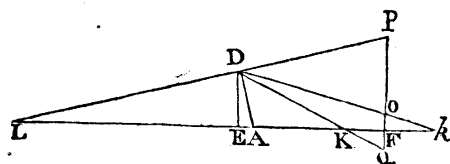
Imperfection
of Telescopes.

Whence they that are skilled in Opticks will easily understand, that the breadth of the least circular space, into which object-glasses of telescopes can collect all sorts of parallel rays, is about the $27\frac{1}{2}$ th part of half the aperture of the glass, or 55th part of the whole aperture^(m); and that the focus of the most refrangible rays is nearer to the object-glass than the focus of the least refrangible ones, by about the $27\frac{1}{2}$ th part of the distance between the object-glass and the focus of the mean refrangible ones.

And if rays of all sorts, flowing from any one lucid point in the axis of any convex lens, be made, by the refraction of the lens, to converge to points not too remote from the lens, the focus of the most refrangible rays shall be nearer to the lens, than the focus of the least refrangible ones, by a distance which is to the

(^m) Lect. Opt. Part I. Prop. xxxvii.

(ⁿ) IMAGINE that Heterogeneous Rays, flowing from a lucid point L, in LA the axis of a spherical lens AD, fall upon the edge of the lens at D; and that they are refracted by the lens so as to meet the axis, the most refrangible in K, the least refrangible in F. Newton affirms that Kk is to $\frac{1}{27.5}$ FA as LF to LA, very nearly; provided the distance FA be not



too great. And this we thus demonstrate. From the point of incidence, D, draw DE perpendicular to the axis of the lens. Through F draw FP parallel with DE; and let FP meet the direct ray, LD, produced in P, and the refracted rays, DK and DK, in o and Q. Now if the distance FA be not vastly great, KF and KF will be very nearly equal; also OF and FQ; OQ will be the least breadth of the circular space, in which the lens DA collects the heterogeneous rays emitted from L, and FO or FQ half that least breadth. (Lect. Opt. Part I. Prop. xxxvii.) But because the lines FO and DE are parallel, therefore KF : KE = FO : DE. Also KF : KE = FQ : DE. Therefore KF + KF or Kk : KE + KE = FO : DE. But KF and KE being equal, KE + KE is twice FE. And if the lens DA be but a small portion of its sphere, twice FE will not sensibly exceed twice FA. Hence Kk : 2FA = FO : DE very nearly. And by consequence Kk : FA = 2FO or OQ : DE. But OQ bears to DE a proportion

the $27\frac{1}{2}$ th part of the distance of the focus of the mean refrangible rays from the lens, as the distance between that focus and the lucid point from whence the rays flow, is to the distance between that lucid point and the lens very nearly⁽ⁿ⁾.

Now to examine whether the difference between the refractions, which the most refrangible and the least refrangible rays, flowing from the same point, suffer in the object-glasses of telescopes and such like glasses, be so great as is here described, I contrived the following Experiment.

Exper. 16. The lens which I used in the second and eighth Experiments, being placed six feet and an inch distant from any object, collected the species of that object, by the mean refrangible rays, at the distance of six feet and an inch from the lens on the other side. And therefore by the foregoing rule it ought to collect the species of that object, by the least refrangible rays, at the distance of six feet and $3\frac{2}{3}$ inches from the lens; and by the most refrangible ones, at the distance of five feet and $10\frac{1}{3}$ inches from it: so that between the two places, where these least and most refrangible rays collect the species, there may be the distance of about $5\frac{1}{3}$ inches. For by that rule, as six feet and an inch (the distance of the lens from the lucid object) is to twelve feet and two inches (the distance of the lucid object from the focus of the mean refrangible rays) that is, as one is to two, so is

portion compounded of the proportions of OQ to FP, and of FP to DE; i. e. of the proportions of OQ to FP, and of LF to LE. But taking the proportions of the sines of refraction of the several rays, which are refrangible in the greatest, least, and mean degree, as they are here stated, it follows, from what is shewn in the xxxviiith Prop. of Lect. Opt. Part I, that OQ bears to FP the proportion of 1 to 27.5. Therefore the proportion of OQ to DE is compounded of the proportions of 1 to 27.5, and of LF to LE. And the proportion of Kk to FA being the same, or very nearly the same with that of OQ to DE, is compounded of these same proportions. That is, Kk : FA = 1 : 27.5 + LF : LE. But again, if the lens DA be a very small portion of a sphere, LE will not be sensibly less than LA, and the proportions of LF to LE and LA will not be sensibly different. Hence Kk : FA = 1 : 27.5 + LF : LA very nearly. That is, Kk : FA = LF : 27.5 LA very nearly. By equality Kk : $\frac{1}{27.5}$ FA = LF : LA very nearly. Q. E. D.

If the distance FA be so great, that KF and FK become sensibly unequal; the proportion of Kk to FA, here stated, will no longer subsist.

Cor. If the lucid point be removed farther and farther from the lens; the proportion of LF and LA, and of consequence that of Kk and $\frac{1}{27.5}$ FA, approaches to equality; and ultimately,

when the ray LD becomes parallel to the axis, LF, LA become equal, and Kk = $\frac{1}{27.5}$ FA, just as

Newton affirms.

the 27 $\frac{1}{2}$ th part of six feet and an inch (the distance between the lens and the same focus) to the distance between the focus of the most refrangible rays and the focus of the least refrangible ones; which is therefore $5\frac{1}{3}$ inches, that is very nearly $5\frac{1}{3}$ inches. Now to know whether this measure was true, I repeated the second and eighth Experiment with coloured light, which was less compounded than that I there made use of: for I now separated the heterogeneous rays one from another, by the method I described in the eleventh Experiment; so as to make a coloured spectrum about twelve or fifteen times longer than broad. This spectrum I cast on a printed book, and placing the abovementioned lens at the distance of six feet and an inch from this spectrum, to collect the species of the illuminated letters at the same distance on the other side; I found that the species of the letters illuminated with Blue were nearer to the lens, than those illuminated with deep Red, by about three inches, or three and a quarter: but the species of the letters illuminated with Indigo and Violet appeared so confused and indistinct, that I could not read them. Whereupon viewing the prism, I found it was full of veins running from one end of the glass to the other; so that the refraction could not be regular. I took another prism therefore which was free from veins; and instead of the letters I used two or three parallel black lines, a little broader than the strokes of the letters; and casting the colours upon these lines, in such manner that the lines ran along the colours from one end of the spectrum to the other; I found the focus where the Indigo, or confine of this colour and Violet, cast the species of the black lines most distinctly, to be about four inches or $4\frac{1}{4}$ nearer to the lens, than the focus where the deepest Red cast the species of the same black lines most distinctly. The Violet was so faint and dark, that I could not discern the species of the lines distinctly by that colour; and therefore considering that the prism was made of a dark coloured glass inclining to Green, I took another prism of clear white glass; but the spectrum of colours, which this prism made, had long white streams of faint light shooting out from both ends of the colours; which made me conclude that something was amiss: and viewing the prism, I found two or three little

little bubbles in the glass which refracted the light irregularly. of Telescopes. Wherefore I covered that part of the glass with black paper; and letting the light pass through another part of it, which was free from such bubbles, the spectrum of colours became free from those irregular streams of light, and was now such as I desired. But still I found the Violet so dark and faint, that I could scarce see the species of the lines by the Violet, and not at all by the deepest part of it, which was next the end of the spectrum. I suspected therefore that this faint and dark colour might be allayed by that scattering light, which was refracted and reflected irregularly, partly by some very small bubbles in the glasses, and partly by the inequalities of their polish: which light, though it was but little, yet it being of a White colour, might suffice to affect the sense so strongly, as to disturb the phenomena of that weak and dark colour the Violet; and therefore I tried, as in the 12th, 13th and 14th Experiments, whether the light of this colour did not consist of a sensible mixture of heterogeneous rays, but found it did not. Nor did the refractions cause any other sensible colour than Violet to emerge out of this light; as they would have done out of White light, and by consequence out of this Violet Light, had it been sensibly compounded with White Light. And therefore I concluded, that the reason why I could not see the species of the lines distinctly by this colour, was only the darkness of this colour and thinness of its light, and its distance from the axis of the lens; I divided therefore those parallel black lines into equal parts, by which I might readily know the distances of the colours in the spectrum from one another; and noted the distances of the lens from the foci of such colours as cast the species of the lines distinctly: and then considered, whether the difference of those distances bear such proportion to $5\frac{1}{3}$ inches, the greatest difference of the distances which the foci of the deepest Red and Violet ought to have from the lens, as the distance of the observed colours from one another in the spectrum bear to the greatest distance of the deepest Red and Violet measured in the rectilinear sides of the spectrum; that is, to the length of those sides, or excess of the length of the spectrum above its breadth. And my observations were as follows.

Imperfection

When I observed and compared the deepest sensible Red, and the colour in the confine of Green and Blue, which at the rectilinear sides of the spectrum was distant from it half the length of those sides; the focus where the confine of Green and Blue cast the species of the lines distinctly on the paper, was nearer to the lens, than the focus where the Red cast those lines distinctly on it, by about $2\frac{1}{2}$ or $2\frac{3}{4}$ inches. For sometimes the measures were a little greater, sometimes a little less, but seldom varied from one another above $\frac{1}{3}$ of an inch. For it was very difficult to define the places of the foci, without some little errors. Now if the colours distant half the length of the image (measured at its rectilinear sides) give $2\frac{1}{2}$ or $2\frac{3}{4}$ difference of the distances of their foci from the lens, then the colours distant the whole length ought to give 5 or $5\frac{1}{2}$ inches difference of those distances.

But here it is to be noted, that I could not see the Red to the full end of the spectrum, but only to the center of the semicircle which bounded that end, or a little farther; and therefore I compared this Red not with that colour which was exactly in the middle of the spectrum, or confine of Green and Blue, but with that which converged a little more to the Blue than to the Green. And as I reckoned the whole length of the colours not to be the whole length of the spectrum, but the length of its rectilinear sides, so completing the semicircular ends into circles, when either of the observed colours fell within those circles, I measured the distance of that colour from the semicircular end of the spectrum; and subtracting half this distance from the measured distance of the two colours, I took the remainder for their corrected distance; and in these observations set down this corrected distance for the difference of the distances of their foci from the lens. For as the length of the rectilinear sides of the spectrum would be the whole length of all the colours, were the circles, of which (as we shewed) that spectrum consists, contracted and reduced to physical points; so in that case this corrected distance would be the real distance of the two observed colours.

When therefore I farther observed the deepest sensible Red, and that Blue whose corrected distance from it was $\frac{7}{12}$ parts of the length of the rectilinear sides of the spectrum, the difference of the

the distances of their foci from the lens was about $3\frac{1}{4}$ inches: and of Telescopes, as 7 to 12 so is $3\frac{1}{4}$ to $5\frac{1}{2}$.

When I observed the deepest sensible Red, and that Indigo whose corrected distance was $\frac{8}{12}$ or $\frac{2}{3}$ of the length of the rectilinear sides of the spectrum; the difference of the distances of their foci from the lens was about $3\frac{2}{3}$ inches: and as 2 to 3 so is $3\frac{2}{3}$ to $5\frac{1}{2}$.

When I observed the deepest sensible Red, and that deep Indigo whose corrected distance from one another was $\frac{9}{12}$ or $\frac{3}{4}$ of the length of the rectilinear sides of the spectrum; the difference of the distances of their foci from the lens was about 4 inches: and as 3 to 4 so is 4 to $5\frac{1}{3}$.

When I observed the deepest sensible Red, and that part of the Violet next the Indigo, whose corrected distance from the Red was $\frac{10}{12}$ or $\frac{5}{6}$ of the length of the rectilinear sides of the spectrum; the difference of the distances of their foci from the lens was about $4\frac{1}{2}$ inches: and as 5 to 6, so is $4\frac{1}{2}$ to $5\frac{2}{3}$. For sometimes when the lens was advantageously placed, so that its axis respected the Blue, and all things else were well ordered, and the sun shone clear, and I held my eye very near to the paper on which the lens cast the species of the lines, I could see pretty distinctly the species of those lines by that part of the Violet which was next the Indigo; and sometimes I could see them by above half the Violet. For in making these Experiments I had observed, that the species of those colours only appear distinct, which were in or near the axis of the lens: so that if the Blue or Indigo were in the axis, I could see their species distinctly; and then the Red appeared much less distinct than before. Wherefore I contrived to make the spectrum of colours shorter than before; so that both its ends might be nearer to the axis of the lens. And now its length was about $2\frac{1}{2}$ inches, and breadth about $\frac{1}{3}$ or $\frac{1}{4}$ of an inch. Also instead of the black lines on which the spectrum was cast, I made one black line broader than those, that I might see its species more easily; and this line I divided by short cross lines into equal parts, for measuring the distances of the observed colours. And now I could sometimes see the species of this line with its divisions almost as far as the center of.

Imperfection of the semicircular Violet end of the spectrum, and made these farther observations.

When I observed the deepest sensible Red, and that part of the Violet whose corrected distance from it was about $\frac{8}{9}$ parts of the rectilinear sides of the spectrum; the difference of the distances of the foci of those colours from the lens, was one time $4\frac{2}{3}$, another time $4\frac{1}{4}$, another time $4\frac{7}{8}$ inches: and as 8 to 9, so are $4\frac{2}{3}$, $4\frac{1}{4}$, $4\frac{7}{8}$, to $5\frac{1}{4}$, $5\frac{11}{32}$, $5\frac{31}{64}$ respectively.

When I observed the deepest sensible Red, and deepest sensible Violet (the corrected distance of which colours when all things were ordered to the best advantage, and the sun shone very clear, was about $\frac{11}{12}$ or $\frac{15}{16}$ parts of the length of the rectilinear sides of the coloured spectrum) I found the difference of the distances of their foci from the lens sometimes $4\frac{3}{4}$, sometimes $5\frac{1}{4}$, and for the most part 5 inches, or thereabouts: and as 11 to 12, or 15 to 16, so is 5 inches to $5\frac{1}{2}$ or $5\frac{1}{3}$ inches.

And by this progression of Experiments I satisfied myself, that had the light at the very ends of the spectrum been strong enough to make the species of the black lines appear plainly on the paper, the focus of the deepest Violet would have been found nearer to the lens, than the focus of the deepest Red, by about $5\frac{1}{3}$ inches at least. And this is a farther evidence, that the fines of incidence and refraction of the several sorts of rays, hold the same proportion to one another in the smallest refractions which they do in the greatest.

My progress in making this nice and troublesome Experiment I have set down more at large, that they, that shall try it after me, may be aware of the circumspection requisite to make it succeed well. And if they cannot make it succeed so well as I did, they may notwithstanding collect, by the proportion of the distance of the colours of the spectrum to the difference of the distances of their foci from the lens, what would be the success in the more distant colours by a better trial. And yet if they use a broader lens than I did, and fix it to a long straight staff, by means of which it may be readily and truly directed to the colour whose focus is desired; I question not but the Experiment will succeed better with them, than it did with me. For I directed the axis

as

as nearly as I could to the middle of the colours; and then the faint ends of the spectrum, being remote from the axis, cast their species less distinctly on the paper, than they would have done, had the axis been successively directed to them.

Now by what has been said, it is certain that the rays which differ in refrangibility do not converge to the same focus; but if they flow from a lucid point, as far from the lens on one side as their foci are on the other, the focus of the most refrangible rays shall be nearer to the lens than that of the least refrangible, by above the fourteenth part of the whole distance: and if they flow from a lucid point, so very remote from the lens, that before their incidence they may be accounted parallel, the focus of the most refrangible rays shall be nearer to the lens than the focus of the least refrangible, by about the 27th or 28th part of their whole distance from it. And the diameter of the circle in the middle space between those two foci, which they illuminate when they fall there on any plane perpendicular to the axis (which circle is the least into which they can all be gathered) is about the 55th part of the diameter of the aperture of the glass. So that it is a wonder, that Telescopes represent objects so distinct as they do. But were all the rays of light equally refrangible, the error arising only from the sphericity of the figures of glasses would be many hundred times less. For if the object-glass of a Telescope be plano-convex, and the plane side be turned towards the object, and the diameter of the sphere whereof this glass is a segment be called D , and the semidiameter of the aperture of the glass be called s , and the sine of incidence out of glass into air, be to the sine of refraction as 1 to R : the rays, which come parallel to the axis of the glass, shall, in the place where the image of the object is most distinctly made, be scattered all over a little circle, whose diameter is $\frac{Rq}{lq} \times \frac{s \text{ cub.}}{D \text{ quad.}}$ very nearly; as I gather by computing the errors of the rays by the method of Infinite Series, and rejecting the terms whose quantities are inconsiderable (^c). As for instance, if the sine of incidence, 1, be to the sine of refraction, R , as 20 to 31; and if D ,

(^c) Vide Lect. Opt. Part I. Sect. IV. Prop. XXXI. Cor. 7.

the

Imperfection

the diameter of the sphere to which the convex side of the glass is ground, be 100 feet, or 1200 inches; and s , the diameter of the aperture, be two inches: the diameter of the little circle (that is $\frac{Rq \times S \text{ cub.}}{17 \times D \text{ quad.}}$) will be $\frac{31 \times 31 \times 8}{20 \times 20 \times 1200 \times 1200}$ or $\frac{961}{72000000}$ parts of an inch. But the diameter of the little circle, through which these rays are scattered by unequal refrangibility, will be about the 55th part of the aperture of the object-glass, which here is four inches. And therefore the error, arising from the spherical figure of the glass, is to the error arising from the different refrangibility of the rays, as $\frac{961}{72000000}$ to $\frac{4}{55}$, that is as 1 to 5449: and therefore, being in comparison so very little, deserves not to be considered.

But you will say, if the errors caused by the different refrangibility be so very great, how comes it to pass that objects appear through Telescopes so distinct as they do? I answer, it is because the erring rays are not scattered uniformly over all that circular space, but collected infinitely more densely in the center than in any other part of the circle; and in the way from the center to the circumference grow continually rarer and rarer, so as at the circumference to become infinitely rare; and by reason of their rarity are not strong enough to be visible, unless in the center, and very near it. Let ADE (in *fig. 27.*) represent one of those circles, described with the center c and semidiameter AC ; and let BFG be a smaller circle concentric to the former, cutting with its circumference the diameter AC in B ; and bisect AC in N : and, by my reckoning, the density of the light in any place B will be to its density in N , as AB to BC ; and the whole light, within the lesser circle BFG, will be to the whole light within the greater AED, as the excess of the square of AC above the square of AB is to the square of AC . As if BC be the fifth part of AC , the light will be four times denser in B than in N ; and the whole light within the less circle, will be to the whole light within the greater, as nine to twenty-five. Whence it is evident that the light within the less circle, must strike the sense much more strongly, than that faint and dilated light round about, between it and the circumference of the greater.

But

But it is farther to be noted, that the most luminous of the of TELE-
Prismatic Colours are the Yellow and Orange. These affect the SCOPES.
senses more strongly than all the rest together, and next to these in strength are the Red and Green. The Blue, compared with these, is a faint and dark colour; and the Indigo and Violet are much darker and fainter: so that these, compared with the stronger colours, are little to be regarded. The images of objects are therefore to be placed, not in the focus of the mean refrangible rays, which are in the confine of Green and Blue, but in the focus of those rays which are in the middle of the Orange and Yellow; there, where the colour is most luminous and fulgent: that is, in the brightest Yellow, that Yellow which inclines more to Orange than to Green. And by the refraction of these rays (whose sines of incidence and refraction in glass are as 17 and 11) the refraction of glass and crystal for optical uses is to be measured. Let us therefore place the image of the object in the focus of these rays, and all the Yellow and Orange will fall within a circle, which diameter is about the 250th part of the diameter of the aperture of the glass. And if you add the brighter half of the Red (that half which is next the Orange) and the brighter half of the Green (that half which is next the Yellow) about three-fifth-parts of the light of these two colours will fall within the same circle, and two fifth-parts will fall without it round about; and that which falls without will be spread through almost as much more space as that which falls within, and so in the gross be almost three times rarer. Of the other half of the Red and Green (that is of the deep Dark Red and Willow-Green) about one quarter will fall within this circle, and three quarters without; and that which falls without will be spread through about four or five times more space than that which falls within, and so in the gross be rarer; and if compared with the whole light within it, will be about 25 times rarer than all that taken in the gross; or rather more than 30 or 40 times rarer: because the deep Red, in the end of the spectrum of colours made by a prism, is very thin and rare, and the Willow-Green is something rarer than the Orange and Yellow. The light of these colours therefore being so very much rarer than that within the circle, will

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scarce affect the sense; especially since the deep Red and Willow Green of this light, are much darker colours than the rest. And for the same reason the Blue and Violet, being much darker colours than these, and much more rarified, may be neglected. For the dense and bright light of the circle, will obscure the rare and weak light of these dark colours round about it, and render them almost insensible. The sensible image of a lucid point is therefore scarce broader than a circle, whose diameter is the 250th part of the diameter of the aperture of the object-glass of a good telescope; or not much broader, if you except a faint and dark misty light round about it, which a spectator will scarce regard. And therefore in a telescope whose aperture is four inches, and length an hundred feet, it exceeds not 2".45", or 3". And in a telescope whose aperture is two inches, and length 20 or 30 feet, it may be 5" or 6", and scarce above. And this answers well to experience: for some astronomers have found the diameters of the fixed stars, in telescopes of between 20 and 60 feet in length, to be about 5" or 6", or at most 8" or 10" in diameter. But if the eye-glass be tinged faintly with the smoke of a lamp or torch, to obscure the light of the star, the fainter light in the circumference of the star ceases to be visible, and the star (if the glass be sufficiently foiled with smoke) appears something more like a mathematical point. And for the same reason, the enormous part of the light in the circumference of every lucid point ought to be less discernible in shorter telescopes than in longer, because the shorter transmit less light to the eye.

Now that the fixed stars, by reason of their immense distance, appear like points, unless so far as their light is dilated by refraction, may appear from hence; that when the moon passes over them and eclipses them, their light vanishes, not gradually like that of the planets, but all at once; and in the end of the eclipse it returns into sight all at once, or certainly in less time than the second of a minute; the refraction of the moon's atmosphere a little protracting the time in which the light of the star first vanishes, and afterwards returns into sight.

Now if we suppose the sensible image of a lucid point, to be even 250 times narrower than the aperture of the glass: yet this image

image would be still much greater, than if it were only from the spherical figure of the glass. For were it not for the different refrangibility of the rays, its breadth, in an 100 foot telescope whose aperture is 4 inches, would be but $\frac{961}{72000000}$ parts of an inch, as is manifest by the foregoing computation. And therefore in this case the greatest errors, arising from the spherical figure of the glass, would be to the greatest sensible errors, arising from the different refrangibility of the rays, as $\frac{961}{72000000}$ to $\frac{4}{250}$ at most, that is only as 1 to 1200. And this sufficiently shews that it is not the spherical figures of glasses, but the different refrangibility of the rays, which hinders the perfection of telescopes.

There is another argument, by which it may appear, that the different refrangibility of rays is the true cause of the imperfection of telescopes. For the errors of the rays, arising from the spherical figures of object-glasses, are as the cubes of the apertures of the object-glasses; and thence to make telescopes of various lengths magnify with equal distinctness, the apertures of the object-glasses, and the charges or magnifying powers ought to be as the cubes of the square-roots of their lengths; which doth not answer to experience. But the errors of the rays, arising from the different refrangibility, are as the apertures of the object-glasses; and thence to make telescopes of various lengths magnify with equal distinctness, their apertures and charges ought to be as the square roots of their lengths; and this answers to experience, as is well known. For instance, a telescope of 64 feet in length, with an aperture of $2\frac{2}{3}$ inches, magnifies about 120 times, with as much distinctness as one of 2 foot in length, with $\frac{1}{3}$ of an inch aperture, magnifies 15 times.

Now were it not for this different refrangibility of rays, telescopes might be brought to a greater perfection than we have yet described, by composing the object-glass of two glasses with water between them. Let ADFC [in fig. 28.] represent the object-glass composed of two glasses, ABED and BEFC, alike convex on the outsides AGD and CHF, and alike concave on the insides BME, BNE. with water in the concavity BMEN. Let the sine of incidence out of glass into air be as 1 to R, and out of water into air as K to R, and by consequence out of glass into water as 1 to K:

K 2

and

and let the diameter of the sphere to which the convex sides, AGD and CHF, are ground be D; and the diameter of the sphere to which the concave sides, BME and BNE, are ground be to D, as the cube-root of KK-KI to the cube-root of RK-RI: and the refractions on the concave sides of the glasses will very much correct the errors of the refractions on the convex sides, so far as they arise from the sphericity of the figure. And by this means might telescopes be brought to sufficient perfection, were it not for the different refrangibility of several sorts of rays. But by reason of this different refrangibility, I do not yet see any other means of improving telescopes by refractions alone, than that of increasing their lengths; for which end the late contrivance of *Hugenius* seems well accommodated. For very long tubes are cumbersome, and scarce to be readily managed, and by reason of their length are very apt to bend, and shake by bending, so as to cause a continual trembling in the objects, whereby it becomes difficult to see them distinctly: whereas, by his contrivance, the glasses are readily manageable, and the object-glass, being fixed upon a strong upright pole, becomes more steady.

REFLECT-
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Seeing therefore the improvement of telescopes of given lengths by refractions is desperate; I contrived heretofore a perspective by reflexion, using instead of an object-glass a concave metal. The diameter of the sphere, to which the metal was ground concave, was about 25 English inches, and by consequence the length of the instrument about six inches and a quarter. The eye-glass was plano-convex, and the diameter of the sphere to which the convex side was ground was about $\frac{1}{5}$ of an inch, or a little less, and by consequence it magnified between 30 and 40 times. By another way of measuring I found that it magnified about 35 times. The concave metal bore an aperture of an inch and a third part; but the aperture was limited not by an opaque circle, covering the limb of the metal round about, but by an opaque circle placed between the eye-glass and the eye; and perforated in the middle with a little round hole for the rays to pass through to the eye. For this circle by being placed here, stopped much of the erroneous light, which otherwise would have disturbed the vision. By comparing it with a pretty good perspective of
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four feet in length, made with a concave eye-glass, I could read ^{TELESCOPE.} at a greater distance with my own instrument than with the glass. Yet objects appeared much darker in it than in the glass, and that partly because more light was lost by reflexion in the metal, than by refraction in the glass, and partly because my instrument was overcharged. Had it magnified but 30 or 25 times, it would have made the object appear more brisk and pleasant. Two of these I made about 16 years ago, and have one of them still by me, by which I can prove the truth of what I write. Yet it is not so good as at the first. For the concave has been divers times tarnished and cleared again, by rubbing it with very soft leather. When I made these, an artist in *London* undertook to imitate it; but using another way of polishing them than I did, he fell much short of what I had attained to, as I afterwards understood by discoursing with the under workman he had employed. The polish I used was in this manner. I had two round copper-plates each six inches in diameter, the one convex the other concave, ground very true to one another. On the convex I ground the object-metal or concave which was to be polished, till it had taken the figure of the convex, and was ready for a polish. Then I pitched over the convex very thinly, by dropping melted pitch upon it, and warming it to keep the pitch soft, whilst I ground it with the concave copper wetted, to make it spread evenly all over the convex. Thus by working it well, I made it as thin as a groat; and after the convex was cold, I ground it again to give it as true a figure as I could. Then I took putty, which I had made very fine by washing it from all its grosser particles, and laying a little of this upon the pitch, I ground it upon the pitch with the concave copper till it had done making a noise; and then upon the pitch I ground the object-metal with a brisk motion, for about two or three minutes of time, leaning hard upon it. Then I put fresh putty upon the pitch, and ground it again till it had done making a noise, and afterwards ground the object-metal upon it as before. And this work I repeated till the metal was polished, grinding it the last time with all my strength for a good while together, and frequently breathing upon the pitch to keep it moist, without laying on any more fresh putty.
The

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The object-metal was two inches broad, and about one-third part of an inch thick, to keep it from bending. I had two of these metals, and when I had polished them both I tried which was best; and ground the other again, to see if I could make it better, than that which I kept. And thus by many trials I learned the way of polishing, till I made those two reflecting perspectives I spake of above. For this art of polishing will be better learned by repeated practice than by my description. Before I ground the object-metal on the pitch, I always ground the putty on it with the concave copper till it had done making a noise; because if the particles of the putty were not by this means made to stick fast in the pitch, they would by rolling up and down grate and fret the object-metal and fill it full of little holes.

But because metal is more difficult to polish than glass, and is afterwards very apt to be spoiled by tarnishing, and reflects not so much light as glass quick-silvered over does: I would propound to use, instead of the metal, a glass ground concave on the fore-side, and as much convex on the back-side, and quick-silvered over on the convex side. The glass must be every where of the same thickness exactly. Otherwise it will make objects look coloured and indistinct. By such a glass I tried, above five or six years ago, to make a reflecting telescope of four feet in length to magnify about 150 times, and I satisfied myself, that there wants nothing but a good artist to bring the design to perfection. For the glass being wrought by one of our *London* artists after such a manner as they grind glasses for telescopes, though it seemed as well wrought as the object-glasses use to be, yet when it was quick-silvered, the reflexion discovered innumerable inequalities all over the glass. And by reason of these inequalities, objects appeared indistinct in this instrument. For the errors of reflected rays caused by any inequality of the glass, are about six times greater than the errors of refracted rays caused by the like inequalities. Yet by this Experiment I satisfied myself that the reflexion on the concave side of the glass, which I feared would disturb the vision, did no sensible prejudice to it; and by consequence that nothing is wanting to perfect these telescopes, but good workmen, who can grind and polish glasses truly spherical.

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An object-glass of a fourteen-foot telescope, made by an artificer ^{TELESCOPE.} at *London*, I once mended considerably, by grinding it on pitch with putty, and leaning very easily on it in the grinding, lest the putty should scratch it. Whether this way may not do well enough for polishing these reflecting glasses, I have not yet tried. But he that shall try either this, or any other way of polishing, which he may think better, may do well to make his glasses ready for polishing, by grinding them without that violence, where-with our *London* workmen press their glasses in grinding. For by such violent pressure, glasses are apt to bend a little in the grinding, and such bending will certainly spoil their figure. To recommend therefore the consideration of these reflecting glasses, to such artists as are curious in figuring glasses, I shall describe this optical instrument in the following Proposition.

P R O P. VIII. P R O B. II.

To shorten Telescopes.

Let *ABDC* [in *fig. 29.*] represent a glass spherically concave on the fore-side *AB*, and as much convex on the back-side *CD*, so that it be every where of an equal thickness. Let it not be thicker on one side than on the other, lest it make objects appear coloured and indistinct; and let it be very truly wrought and quick-silvered over on the back-side, and set in the tube *VXYZ* which must be very black within. Let *EF**G* represent a prism of glass, or crystal, placed near the other end of the tube, in the middle of it, by means of a handle of brass or iron *FGK*, to the end of which made flat it is cemented. Let this prism be rectangular at *E*, and let the other two angles at *F* and *G* be accurately equal to each other, and by consequence equal to half right ones; and let the plane sides *FE* and *GE* be square, and by consequence the third side *FG* a rectangular parallelogram, whose length is to its breadth in a subduplicate proportion of two to one. Let it be so placed in the tube, that the axis of the speculum may pass through the middle of the square side *EF* perpendicularly, and by consequence through the middle of the side *FG* at an angle of 45 degrees; and let the side *EF* be turned towards the speculum, and the:

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the distance of this prism from the speculum be such, that the rays of the light PQ, RS, &c. which are incident upon the speculum in lines parallel to the axis thereof, may enter the prism at the side EF, and be reflected by the side FG, and thence go out of it, through the side GE, to the point T; which must be the common focus of the speculum ABDC, and of a plano-convex eye-glass, H, through which those rays must pass to the eye. And let the rays, at their coming out of the glass, pass through a small round hole or aperture made in a little plate of lead, brass or silver, wherewith the glass is to be covered, which hole must be no bigger than is necessary for light enough to pass through. For so it will render the object distinct, the plate, in which it is made, intercepting all the erroneous part of the light which comes from the verges of the speculum AB. Such an instrument well made, if it be six foot long (reckoning the length from the speculum to the prism, and thence to the focus T) will bear an aperture of six inches at the speculum, and magnify between two and three hundred times. But the hole H here limits the aperture with more advantage, than if the aperture was placed at the speculum. If the instrument be made longer or shorter, the aperture must be in proportion as the cube of the square-root of the length, and the magnifying as the aperture. But it is convenient that the speculum be an inch or two broader than the aperture at least, and that the glass of the speculum be thick, that it bend not in the working. The prism EFG must be no bigger than is necessary, and its backside FG must not be quick-silvered over. For without quick-silver it will reflect all the light incident on it from the speculum.

In this instrument the object will be inverted, but may be erected by making the square sides EF and EG of the prism EFG not plane but spherically convex, that the rays may cross as well before they come at it, as afterwards between it and the eye-glass. If it be desired that the instrument bear a larger aperture, that may be also done by composing the speculum of two glasses with water between them.

If the theory of making telescopes could at length be fully brought into practice, yet there would be certain bounds beyond which

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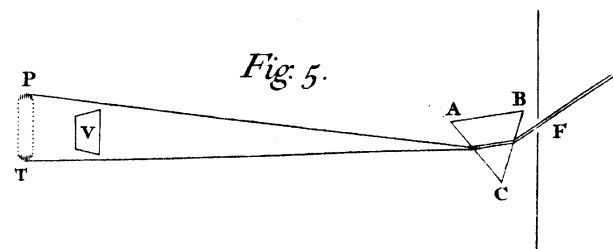
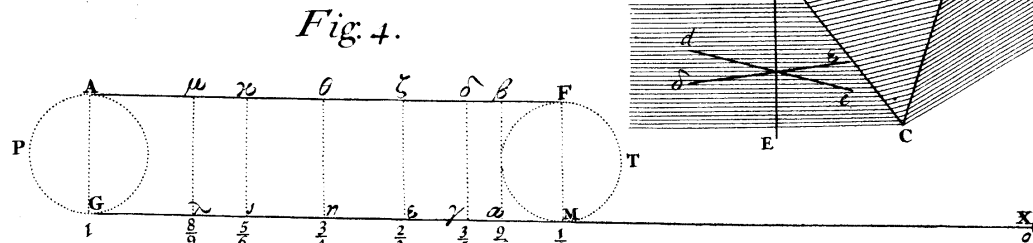
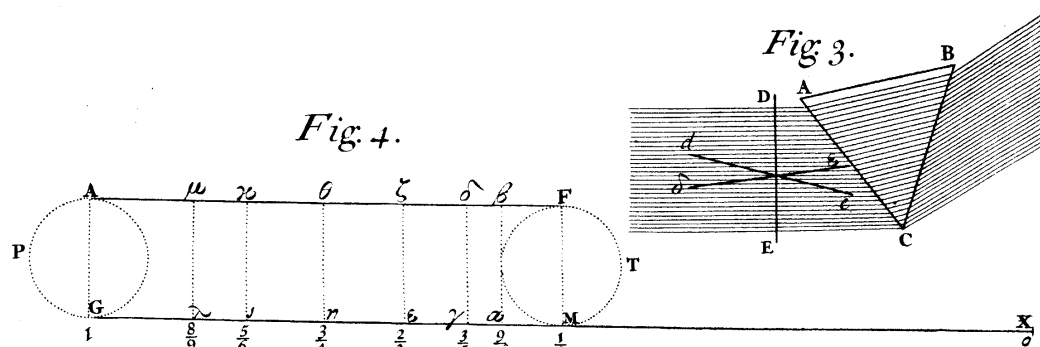
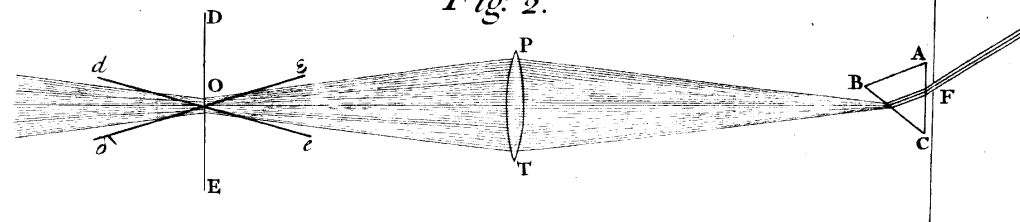
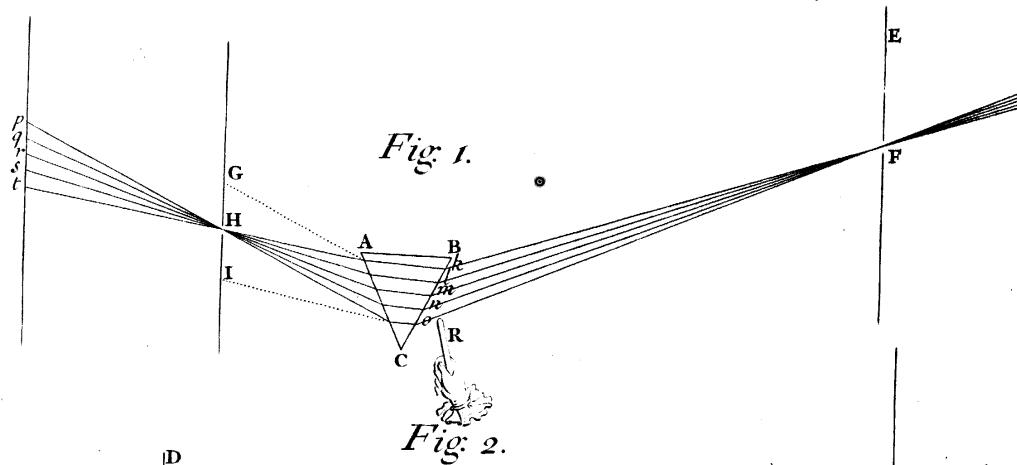


Fig. 6.

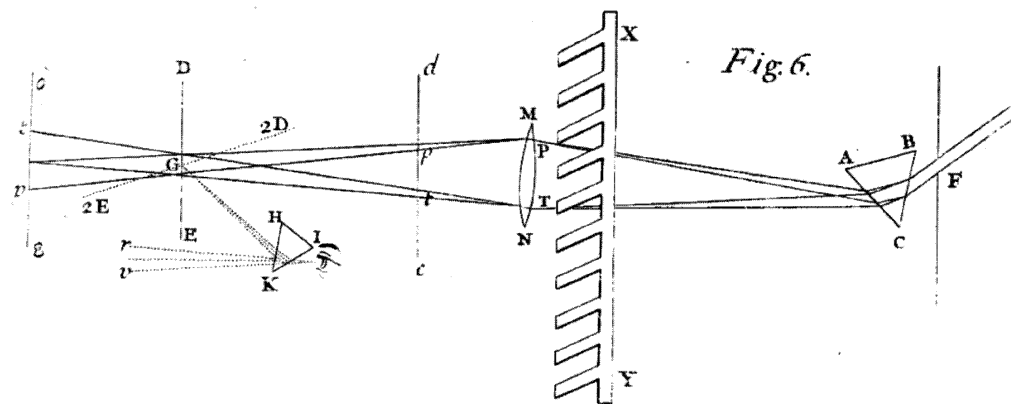


Fig. 7.

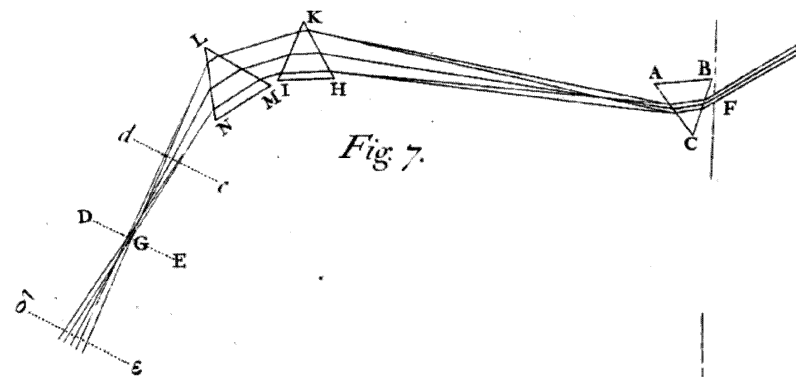
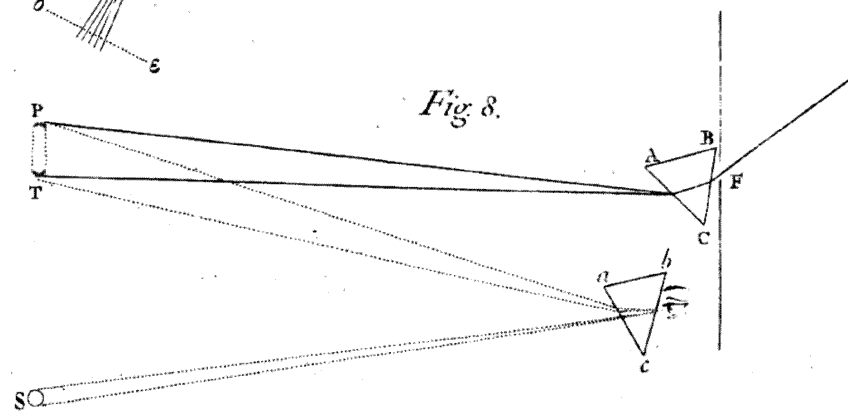


Fig. 8.



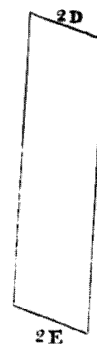


Fig 9.

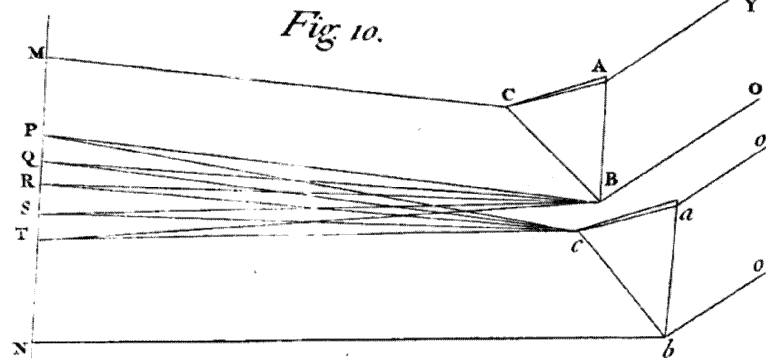
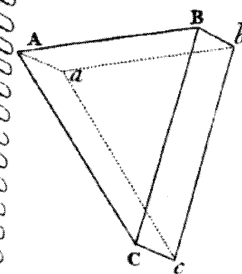
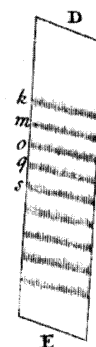


Fig 10.

Fig 11.

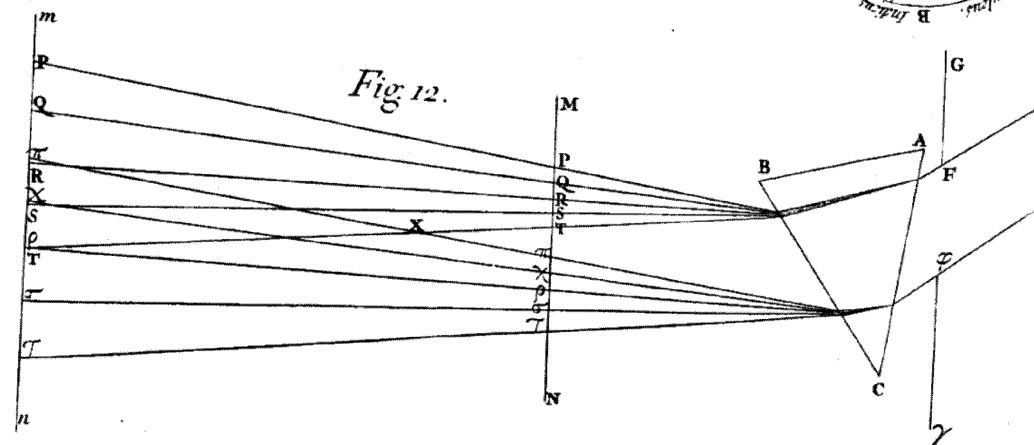


Fig 12.

Fig 13.

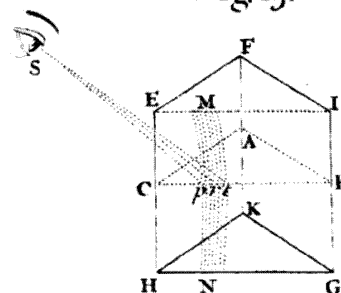


Fig 14.

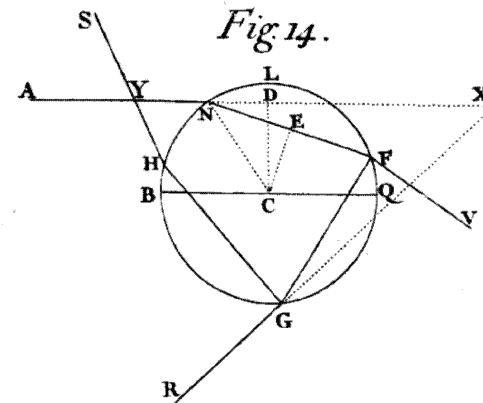


Fig 15.

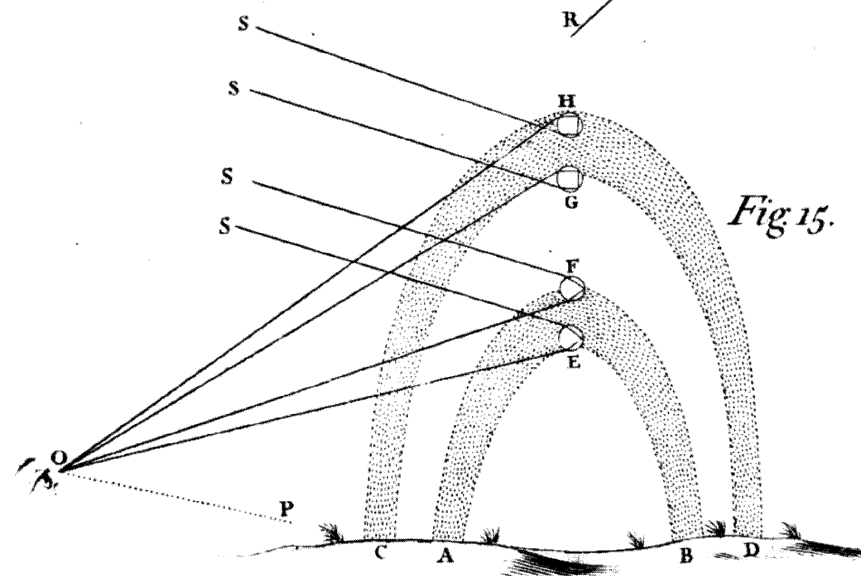


Fig 16.

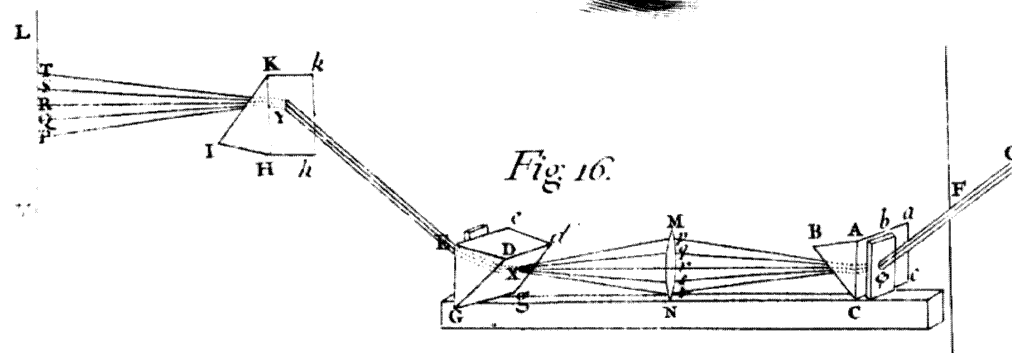


Fig. 17.

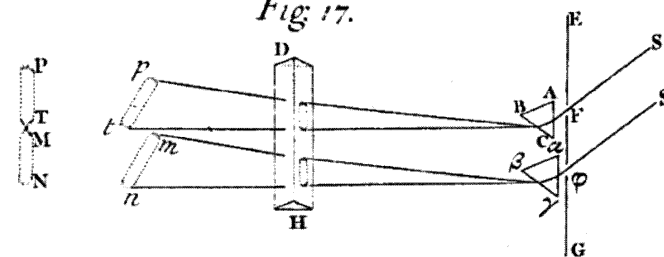


Fig. 18.

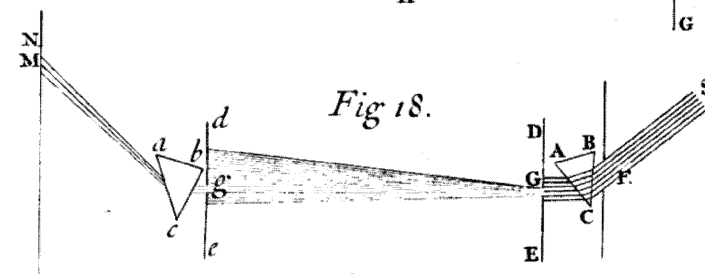


Fig. 19.

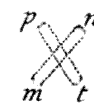
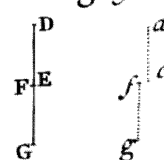


Fig. 20.

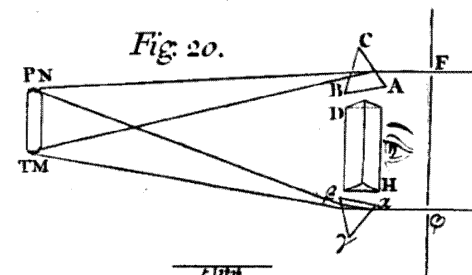


Fig. 21.

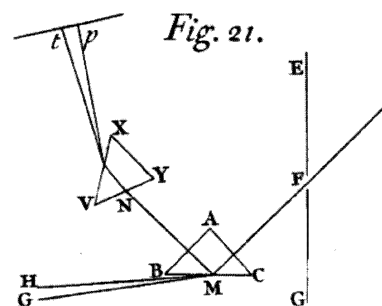
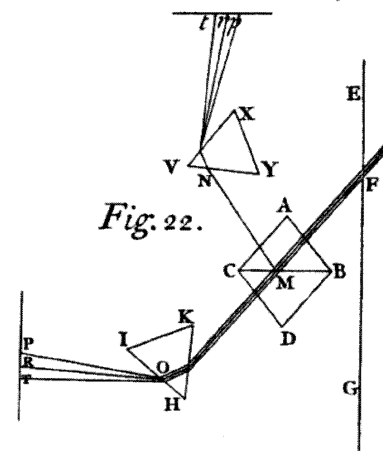
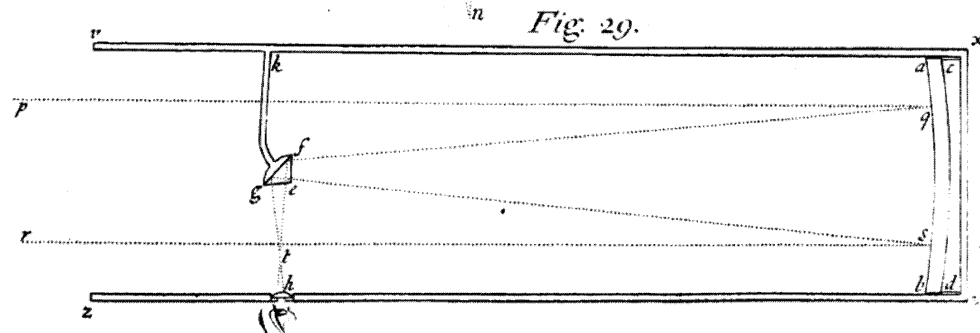
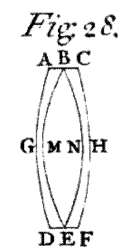
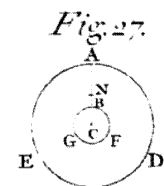
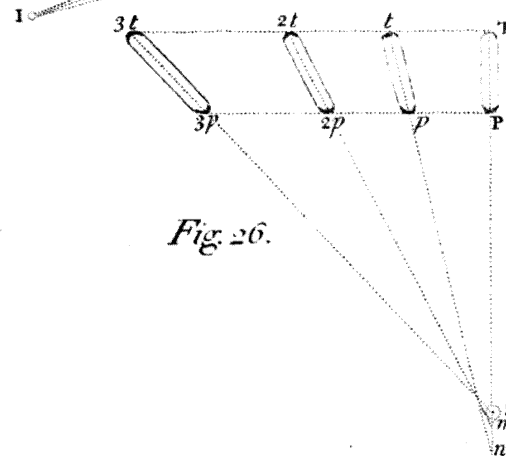
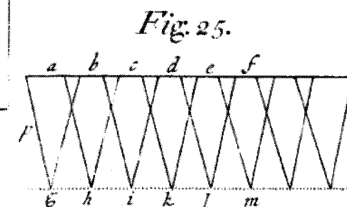
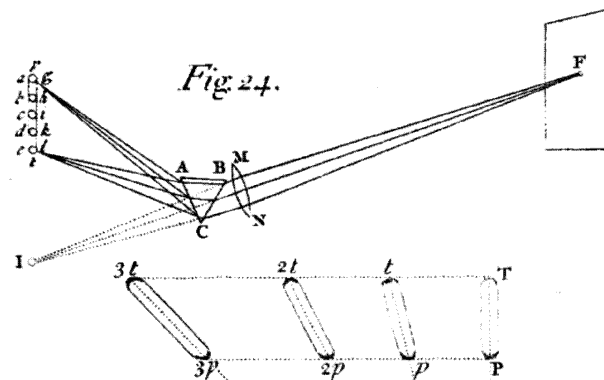
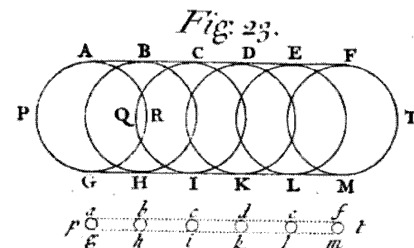


Fig. 22.





which telescopes could not perform. For the air, through which we look upon the stars, is in a perpetual tremor; as may be seen by the tremulous motion of shadows cast from high towers, and by the twinkling of the fixed stars. But these stars do not twinkle when viewed through telescopes which have large apertures. For the rays of light, which pass through divers parts of the aperture, tremble each of them apart; and by means of their various and sometimes contrary tremors, fall at one and the same time upon different points in the bottom of the eye; and their trembling motions are too quick and confused to be perceived severally. And all these illuminated points constitute one broad lucid point, composed of those many trembling points confusedly and insensibly mixed with one another by very short and swift tremors; and thereby cause the star to appear broader than it is, and without any trembling of the whole. Long telescopes may cause objects to appear brighter and larger than short ones can do; but they cannot be so formed as to take away that confusion of the rays, which arises from the tremors of the atmosphere. The only remedy is a most serene and quiet air, such as may perhaps be found on the tops of the highest mountains above the grosser clouds.

T H E

F I R S T B O O K

O F

O P T I C S.

P A R T II.

P R O P. I. T H E O R. I.

The phenomena of colours in Refracted or Reflected Light are not caused by new modifications of the light variously impressed, according to the various terminations of the light and shadow.

The proof by Experiments.

Exper. 1. **F**OR if the sun shine into a very dark chamber through an oblong hole F [in *fig. 1.*] whose breadth is the sixth or eighth part of an inch, or something less; and his beam, FH, do afterwards pass first through a very large prism ABC, distant about 20 feet from the hole, and parallel to it, and then (with its white part) through an oblong hole H, whose breadth is about the fortieth or fiftieth part of an inch, and which is made in a black opake body GI, and placed at the distance of two or three feet from the prism, in a parallel situation both to the

the

PART II.

O P T I C S.

the prism and to the former hole, and if this White light thus transmitted through the hole H, fall afterwards upon a white paper *pt*, placed after that hole H, at the distance of three or four feet from it, and there paint the usual colours of the prism, suppose Red at *t*, Yellow at *s*, Green at *r*, Blue at *q*, and Violet at *p*; you may with an iron wire, or any such like slender opake body, whose breadth is about the tenth part of an inch, by intercepting the rays at *k*, *l*, *m*, *n* & *o*, take away any one of the colours at *t*, *s*, *r*, *q* or *p*, whilst the other colours remain upon the paper as before; or with an obstacle something bigger you may take away any two, or three, or four colours together, the rest remaining. So that any one of the colours, as well as Violet, may become outmost in the confine of the shadow towards *p*, and any one of them as well as Red may become outmost in the confine of the shadow towards *t*, and any one of them may also border upon the shadow made within the colours by the obstacle, *r*, intercepting some intermediate part of the light; and, lastly, any one of them, by being left alone, may border upon the shadow on either hand. All the colours have themselves indifferently to any confines of shadow, and therefore the differences of these colours from one another, do not arise from the different confines of shadow, whereby light is variously modified, as has hitherto been the opinion of philosophers. In trying these things it is to be observed, that by how much the holes, F and H, are narrower, and the intervals between them and the prism greater, and the chamber darker, by so much the better doth the experiment succeed; provided the light be not so far diminished, but that the colours at *pt* be sufficiently visible. To procure a prism of solid glass large enough for this Experiment will be difficult, and therefore a prismatick vessel must be made of polished glass plates cemented together, and filled with salt water or clear oil.

Exper. 2. The sun's light let into a dark chamber through the round hole F [in *fig. 2.*] half an inch wide, passed first through the prism ABC placed at the hole; and then through a lens, PT, something more than four inches broad, and about eight feet distant from the prism; and thence converged to *o* the focus of the lens distant from it about three feet; and there fell upon a

L 2

white

Colour no
reflect

white paper *DE*. If that paper was perpendicular to that light incident upon it, as it is represented in the posture *DE*, all the colours upon it, at *o*, appeared White. But if the paper being turned about an axis parallel to the prism, became very much inclined to the light, as it is represented in the positions *de* and *δe*; the same light in the one case appeared Yellow and Red, in the other Blue. Here one and the same part of the light, in one and the same place, according to the various inclinations of the paper, appeared in one case White, in another Yellow or Red, in a third Blue; whilst the confine of light and shadow, and the refractions of the prism, in all these cases, remained the same.

Exper. 3. Such another Experiment may be more easily tried as follows. Let a broad beam of the sun's light, coming into a dark chamber through a hole in the window-shut, be refracted by a large prism *ABC* [in *fig. 3.*] whose refracting angle *c* is more than 60 degrees; and so soon as it comes out of the prism, let it fall upon the white paper, *DE*, glewed upon a stiff plane; and this light, when the paper is perpendicular to it, as it is represented in *DE*, will appear perfectly White upon the paper; but when the paper is very much inclined to it, in such a manner as to keep always parallel to the axis of the prism, the whiteness of the whole light upon the paper will, according to the inclination of the paper this way or that way, change either into Yellow and Red, as in the posture *de*; or into Blue and Violet, as in the posture *δe*. And if the light, before it fall upon the paper, be twice refracted the same way by two parallel prisms, these colours will become the more conspicuous. Here all the middle parts of the broad beam of white light, which fell upon the paper, did, without any confine of shadow to modify it, become coloured all over with one uniform colour; the colour being always the same in the middle of the paper as at the edges; and this colour changed according to the various obliquity of the reflecting paper, without any change in the refractions or shadow, or in the light, which fell upon the paper. And therefore these colours are to be derived from some other cause, than the new modifications of light by refractions and shadows.

If it be asked, What then is their cause? I answer, that the paper, in the posture *de*, being more oblique to the more refrangible rays than to the less refrangible ones, is more strongly illuminated by the latter than by the former; and therefore the less refrangible rays are predominant in the reflected light. And wherever they are predominant in any light they tinge it with Red or Yellow, as may in some measure appear by the first Proposition of the first Book, and will more fully appear hereafter. And the contrary happens in the posture of the paper *δe*, the more refrangible rays being then predominant, which always tinge light with Blues and Violets.

Exper. 4. The colours of bubbles with which children play are various, and change their situation variously, without any respect to any confine of shadow. If such a bubble be covered with a concave glass, to keep it from being agitated by any wind or motion of the air, the colours will slowly and regularly change their situation, even whilst the eye, and the bubble, and all bodies which emit any light, or cast any shadow, remain unmoved. And therefore their colours arise from some regular cause, which depends not on any confine of shadow. What this cause is, will be shewed in the next Book.

To these Experiments may be added the tenth Experiment of the first Book; where the sun's light in a dark room being trajected through the parallel superficies of two prisms tied together in the form of a parallelepiped, became totally of one uniform Yellow or Red colour, at its emerging out of the prisms. Here, in the production of these colours, the confine of shadow can have nothing to do. For the light changes from White to Yellow, Orange and Red successively, without any alteration of the confine of shadow: and at both edges of the emerging light, where the contrary confines of shadow ought to produce different effects, the colour is one and the same, whether it be White, Yellow, Orange or Red: and in the middle of the emerging light, where there is no confine of shadow at all, the colour is the very same as at the edges, the whole light at its very first emergence being of one uniform colour, whether White, Yellow, Orange or Red, and going on thence perpetually without any change of colour,

Colour no effect of confine of Light and Shade, four, such as the confine of shadow is vulgarly supposed to work in refracted light after its emergence. Neither can these colours arise from any new modifications of the light by refractions; because they change successively from White to Yellow, Orange and Red, while the refractions remain the same; and also because the refractions are made contrary ways by parallel superficies, which destroy one another's effects. They arise not therefore from any modifications of light made by refractions and shadows, but have some other cause. What that cause is we shewed above in this tenth Experiment, and need not here repeat it.

There is yet another material circumstance of this Experiment. For this emerging light being by a third prism *HIK* [in *fig. 22. Part I.*] refracted towards the paper *PT*, and there painting the usual colours of the prism, Red, Yellow, Green, Blue, Violet: if these colours arose from the refractions of that prism modifying the light, they would not be in the light before its incidence on that prism. And yet in that Experiment we found, that when, by turning the two first prisms about their common axis, all the colours were made to vanish but the Red; the light which makes that Red being left alone, appeared of the very same Red colour before its incidence on the third prism. And in general we find by other Experiments, that when the rays, which differ in refrangibility, are separated from one another, and any one sort of them is considered apart; the colour of the light, which they compose, cannot be changed by any refraction or reflexion whatever; as it ought to be, were colours nothing else than modifications of light caused by refractions, and reflexions, and shadows. This unchangeableness of colour I am now to describe in the following Proposition.

P R O P. II. T H E O R. II.

All Homogeneous Light has its proper colour answering to its degree of refrangibility, and that colour cannot be changed by reflexions and refractions.

The colours
of Homogeneous

In the Experiments of the fourth Proposition of the first Book, when I had separated the Heterogeneous Rays from one another, the

the spectrum *pt*, formed by the separated rays, did in the progress from its end *p*, on which the most refrangible rays fell, unto its other end *t*, on which the least refrangible rays fell, appear tinged with this series of colours; Violet, Indigo, Blue, Green, Yellow, Orange, Red, together with all their intermediate degrees in a continual succession perpetually varying. So that there appeared as many degrees of colours, as there were sorts of rays differing in refrangibility.

Exper. 5. Now that these colours could not be changed by refraction, I knew by refracting with a prism sometimes one very little part of this light, sometimes another very little part, as is described in the twelfth Experiment of the first Book. For by this refraction the colour of the light was never changed in the least. If any part of the Red light was refracted, it remained totally of the same Red colour as before. No Orange, no Yellow, no Green or Blue, no other new colour was produced by that refraction. Neither did the colour any ways change by repeated refractions, but continued always the same Red entirely as at first. The like constancy and immutability I found also in the Blue, Green, and other colours. So also if I looked through a prism upon any body illuminated with any part of this Heterogeneous light, as in the fourteenth Experiment of the first Book is described; I could not perceive any new colour generated this way. All bodies illuminated with Compound light appear through prisms confused (as was said above) and tinged with various new colours; but those illuminated with Homogeneous light appeared through prisms neither less distinct, nor otherwise coloured, than when viewed with the naked eyes. Their colours were not in the least changed by the refraction of the interposed prism. I speak here of a sensible change of colour: for the light which I here call Homogeneous, being not absolutely homogeneous, there ought to arise some little change of colour from its heterogeneity. But if that heterogeneity was so little, as it might be made by the said Experiments of the fourth Proposition, that change was not sensible; and therefore in Experiments, where sense is judge, ought to be accounted none at all.

Exper. 6. And as these colours were not changeable by refractions, so neither were they by reflexions. For all White, Grey, Red, Yellow, Green, Blue, Violet bodies, as Paper, Ashes, Red Lead, Orpiment, Indigo, Bise, Gold, Silver, Copper, Grass, Blue Flowers, Violets, bubbles of Water tinged with various colours, Peacock's Feathers, the tincture of *Lignum Nephriticum*, and such like, in Red Homogeneous Light appeared totally red; in Blue light totally blue; in Green light totally green; and so of other colours. In the Homogeneous light of any colour they all appeared totally of that same colour; with this only difference, that some of them reflected that light more strongly, others more faintly. I never yet found any body, which by reflecting Homogeneous light, could sensibly change its colour.

From all which it is manifest, that if the sun's light consisted of but one sort of rays, there would be but one colour in the whole world; nor would it be possible to produce any new colour by reflexions and refractions, and by consequence that the variety of colours depends upon the composition of light.

D E F I N I T I O N.

The homogeneous light and rays which appear red, or rather make objects appear so, I call Rubrific or Red-making; those which make objects appear yellow, green, blue and violet, I call Yellow-making, Green-making, Blue-making, Violet-making; and so of the rest. And if at any time I speak of light and rays as coloured, or endued with colours; I would be understood to speak not philosophically and properly, but grossly, and according to such conceptions as vulgar people, in seeing all these Experiments, would be apt to frame. For the rays, to speak properly, are not coloured. In them there is nothing else, than a certain power and disposition to stir up a sensation of this or that colour. For as found in a bell, or musical string, or other sounding body, is nothing but a trembling motion; and in the air, nothing but that motion propagated from the object; and in the sensorium, it is a sense of that motion under the form of sound; so Colours in the Object are nothing but a disposition to reflect this or that sort of rays more copiously than the rest; in the Rays, they are nothing but

but their dispositions to propagate this or that motion into the sensorium; and in the sensorium, they are sensations of those motions under the forms of colours.

P R O P. III. P R O B. I.

To define the refrangibility of the several sorts of Homogeneous Light answering to the several colours.

For determining this Problem I made the following Experiment. The refrangibility of the several Rays defined.

Exper. 7. When I had caused the rectilinear sides *AF*, *GM* [in *fig. 4.*] of the spectrum of colours made by the prism to be distinctly defined, as in the fifth Experiment of the first Part is described (^a), there were found in it all the Homogeneous colours, in the same order and situation one among another as in the spectrum of simple light, described in the fourth Proposition of that Part. For the circles, of which the spectrum of Compound Light *PT* is composed, and which in the middle parts of the spectrum interfere, and are intermixed with one another, are not intermixed in their outmost parts, where they touch those rectilinear sides *AF* and *GM*. And therefore in those rectilinear sides, when distinctly defined, there is no new colour generated by refraction. I observed also, that if any where between the two outmost circles, *TME* and *PGA*, a right line, as *γδ*, was cross to the spectrum, so as at both ends to fall perpendicularly upon its rectilinear sides, there appeared one and the same colour, and degree of colour from one end of this line to the other. I delineated therefore in a paper the perimeter of the spectrum *PABGMT*, and in trying the third Experiment of the first Book, I held the paper so that the spectrum might fall upon this delineated figure, and agree with it exactly; whilst an assistant, whose eyes for distinguishing colours were more critical than mine, did by right lines *αβ*, *γδ*, *εζ*, &c. drawn cross the spectrum, note the confines of the colours; that is, of the Red *MXEF*; of the Orange, *αγδβ*; of the Yellow, *γεζδ*; of the Green, *εηζζ*; of the Blue, *ημδ*; of the Indigo,

(^a) Namely, by placing a lens at the hole where the light was admitted, to take away the penumbra in the sides of the image. Vid. p. 33.

The refrangibility of the $\lambda\mu\kappa$; and of the Violet, $\lambda G A \mu$. And this operation being divers times repeated, both in the same and in several papers, I found that the observations agreed well enough with one another; and that the rectilinear sides, MG and FA , were by the said cross lines divided after the manner of a Musical Chord. Let GM be produced to x , that MX may be equal to GM , and conceive ox , λx , ιx , ηx , ϵx , γx , αx , MX , to be in proportion to one another, as the numbers, $1, \frac{8}{5}, \frac{5}{3}, \frac{3}{2}, \frac{2}{3}, \frac{3}{5}, \frac{9}{16}, \frac{1}{2}$, and so to represent the chords of the key, and of a tone, a third minor, a fourth, a fifth, a sixth major, a seventh and an eighth above that key: and the intervals $M\alpha$, $\alpha\gamma$, $\gamma\epsilon$, $\epsilon\eta$, $\eta\iota$, $\iota\lambda$, and λG , will be the species which the several colours (Red, Orange, Yellow, Green, Blue, Indigo, Violet) take up ^(b).

Now these intervals or spaces subtending the differences of the refractions of the rays going to the Limits of those colours, that is, to the points M , α , γ , ϵ , η , ι , λ , G , may, without any sensible error, be accounted proportional to the differences of the sines of refraction of those rays having one common sine of incidence; and therefore, since the common sine of incidence of the most and least refrangible rays out of glass into air, was by a method described above, found in proportion to their sines of refraction, as 50 to 77 and 78; divide the difference between the sines of refraction, 77 and 78, as the line GM is divided by those intervals; and you will have 77, $77\frac{1}{8}$, $77\frac{1}{3}$, $77\frac{1}{3}$, $77\frac{1}{3}$, $77\frac{1}{3}$, $77\frac{2}{9}$, 78, the sines of refraction of those rays out of glass into air, their common sine of incidence being 50 ^(c). So then the sines of the incidences of all the Red-making rays out of glass into air, were to the sines of their refractions, not greater than 50 to 77, nor less than 50 to $77\frac{1}{8}$; but they varied from one another according to all intermediate proportions. And the sines of the incidences of the Green-making rays were to the sines of their refractions in all proportions from that of 50 to $77\frac{1}{3}$, unto that of 50 to $77\frac{1}{8}$. And

^(b) Vid. Lect. Opt. Part II. § 119—123.

^(c) Lect. Opt. Part II. § 124—127.

^(d) — that it emergeth in lines parallel to those in which it was incident.] Rather, that the several colorific rays emerge altogether, in a line parallel to that in which the compound ray was incident. For the Author's meaning is, that if succeeding refractions collect the colorific rays, which were separated one from another by the first, into a single point upon the last surface, from which they

And by the like limits abovementioned were the refractions of the several Rays belonging to the rest of the colours defined; the sines of the Red-making rays extending from 77 to $77\frac{1}{8}$; those of the Orange-making, from $77\frac{1}{8}$ to $77\frac{1}{3}$; those of the Yellow-making, from $77\frac{1}{3}$ to $77\frac{1}{3}$; those of the Green-making, from $77\frac{1}{3}$ to $77\frac{1}{3}$; those of the Blue-making, from $77\frac{1}{3}$ to $77\frac{2}{9}$; those of the Indigo-making, from $77\frac{2}{9}$ to $77\frac{2}{9}$, and those of the Violet from $77\frac{2}{9}$ to 78.

These are the laws of the refractions made out of glass into air, and thence by the third Axiom of the first Part of this Book, the laws of the refractions made out of air into glass are easily derived.

Exper. 8. I found moreover that when light goes out of air through several contiguous refracting mediums, as through water and glass, and thence goes out again into air, whether the refracting superficies be parallel or inclined to one another, that light as often as by contrary refractions it is so corrected, that it emergeth in lines parallel to those in which it was incident ^(d), continues ever after to be White. But if the emergent rays be inclined to the incident, the whiteness of the emerging light will by degrees in passing on from the place of emergence, become tinged in its edges with colours. This I tried by refracting light with prisms of glass placed within a prismatick vessel of water. Now those colours argue a diverging and separation of the heterogeneous rays from one another by means of their unequal refractions; as in what follows will more fully appear. And on the contrary, the permanent Whiteness argues, that in like incidences of the rays there is no such separation of the emerging rays, and by consequence no inequality of their whole refractions. Whence I seem to gather the two following Theorems.

I. The excesses of the sines of refraction of several sorts of rays above their common sine of incidence, when the refractions are made out of divers denser mediums immediately into one and

they are to emerge, and send them out from thence in a direction parallel to that, in which the compound ray was incident; the emergent light will ever after, unless resolved again by some new refraction, continue to be white. If the colorific rays emerge from different points in the last refracting surface, notwithstanding that they go forth in parallel directions, each will preserve its proper colour: as our Author hath fully shewn in his Lect. Opt. Part II. Sect. iv.

The refrangibility of the same rarer medium, suppose of air, are to one another in a given proportion (e).

II. The proportion of the sine of incidence to the sine of refraction of one and the same sort of rays out of one medium into another, is composed of the proportion of the sine of incidence to the sine of refraction out of the first medium into any third medium, and of the proportion of the sine of incidence to the sine of refraction out of that third medium into the second medium (f).

By the first Theorem the refractions of the rays of every sort, made out of any medium into air, are known by having the refraction of the rays of any one sort. As for instance, if the refractions of the rays of every sort out of rain-water into air be desired, let the common sine of incidence out of glass into air be subducted from the sines of refraction, and the excesses will be $27, 27\frac{1}{3}, 27\frac{1}{3}, 27\frac{1}{3}, 26\frac{1}{3}, 27\frac{1}{3}, 27\frac{1}{3}, 28$. Suppose now that the sine of incidence of the least refrangible rays be to their sine of refraction out of rain-water into air as 3 to 4; and say as 1, the difference of those sines, is to 3, the sine of incidence, so is 27, the least of the excesses above-mentioned, to a fourth number, 81; and 81 will be the common sine of incidence out of rain-water into air; to which sine if you add all the above-mentioned excesses, you will have the desired sines of the refractions $108, 108\frac{1}{3}, 108\frac{1}{3}, 108\frac{1}{3}, 108\frac{1}{3}, 108\frac{2}{3}, 108\frac{2}{3}, 109$.

By the latter Theorem the refraction out of one medium into another is gathered, as often as you have the refractions out of them both into any third medium. As if the sine of incidence of any ray out of glass into air be to its sine of refraction as 20 to 31, and the sine of incidence of the same ray out of air into water be to its sine of refraction as 4 to 3; the sine of incidence of that ray out of glass into water will be to its sine of refraction as 20 to 31, and 4 to 3 jointly; that is, as the factum of 20 and 4 to the factum of 31 and 3, or as 80 to 93.

And these Theorems being admitted into Opticks, there would be scope enough of handling that science voluminously after a new manner; not only by teaching those things which tend to the

(f) Vide Lect. Opt. Part. I. § 44.

perfection

perfection of vision, but also by determining mathematically all kinds of phenomena of colours which could be produced by refractions. For to do this, there is nothing else requisite than to find out the separations of heterogeneous rays, and their various mixtures, and proportions in every mixture. By this way of arguing I invented almost all the phenomena described in these books, beside some others less necessary to the argument; and by the successes I met with in the trials, I dare promise, that to him who shall argue truly, and then try all things with good glasses and sufficient circumspection, the expected event will not be wanting. But he is first to know what colours will arise from any others mixed in any assigned proportion.

PROP. IV. THEOR. III.

Colours may be produced by composition, which shall be like to the colours of Homogeneous Light as to the appearance of colour, but not as to the immutability of colour and constitution of light. And those colours, by how much they are more compounded, by so much are they less full and intense; and, by too much composition, they may be diluted and weakened till they cease, and the mixture becomes white or grey. There may be also colours produced by composition, which are not fully like any of the colours of Homogeneous Light.

For a mixture of Homogeneous Red and Yellow compounds an Orange, like in appearance of colour to that Orange, which in the series of unmixed prismatic colours lies between them; but the light of one Orange is Homogeneous as to refrangibility, that of the other is Heterogeneous; and the colour of the one, if viewed through a prism, remains unchanged; that of the other is changed and resolved into its component colours, Red and Yellow. And after the same manner other neighbouring homogeneous colours may compound new colours, like the intermediate homogeneous ones; as Yellow and Green, the colour between them both; and afterwards, if Blue be added, there will be made a Green, the middle colour of the three which enter the composition.

(f) Vide Lect. Opt. Part. I. § 34 & 35.

tion :

Colours
made by
Composition.

tion : for the Yellow and Blue on either hand, if they are equal in quantity, they draw the intermediate Green equally towards themselves in composition, and so keep it as it were *in æquilibrio*, that it verge not more to the Yellow on the one hand, than to the Blue on the other, but by their mixed actions remain still a middle colour. To this mixed Green there may be farther added some Red and Violet ; and yet the Green will not presently cease, but only grow less full and vivid ; and by increasing the Red and Violet it will grow more and more dilute, until, by the prevalence of the added colours, it be overcome and turned into whiteness, or some other colour. So if to the colour of any homogeneous light, the sun's white light composed of all sorts of rays be added, that colour will not vanish, or change its species, but be diluted ; and, by adding more and more White, it will be diluted more and more perpetually. Lastly, if Red and Violet be mingled, there will be generated, according to their various proportions, various Purples, such as are not like in appearance to the colour of any Homogeneous Light ; and of these Purples mixed with Yellow and Blue may be made other new colours.

P R O P. V. T H E O R. IV.

Whiteness, and all Grey colours between white and black, may be compounded of colours, and the whiteness of the sun's light is compounded of all the Primary colours mixed in a due proportion.

The Proof by Experiments.

The whiteness
of the Solar
Light

Exper. 9. The sun shining into a dark chamber through a little round hole in the window-shut, and his light being there refracted by a prism to cast his coloured image *PT* [in *fig. 5.*] upon the opposite wall : I held a white paper, *v*, to that image, in such manner that it might be illuminated by the coloured light reflected from thence, and yet not intercept any part of that light in its passage from the prism to the spectrum. And I found that when the paper was held nearer to any colour than to the rest, it appeared of that colour to which it approached nearest ; but when it was equally or almost equally distant from all the colours, so that

that it might be equally illuminated by them all, it appeared White. And in this last situation of the paper, if some colours were intercepted, the paper lost its White colour, and appeared of the colour of the rest of the light which was not intercepted. So then the paper was illuminated with lights of various colours, namely, Red, Yellow, Green, Blue and Violet ; and every part of the light retained its proper colour, until it was incident on the paper, and became reflected thence to the eye ; so that if it had been either alone, the rest of the light being intercepted, or if it had abounded most and been predominant in the light reflected from the paper, it would have tinged the paper with its own colour ; and yet being mixed with the rest of the colours in a due proportion, it made the paper look White, and therefore by a composition with the rest produced that colour. The several parts of the coloured light reflected from the spectrum, whilst they are propagated from thence through the air, do perpetually retain their proper colours ; because wherever they fall upon the eyes of any spectator, they make the several parts of the spectrum to appear under their proper colours. They retain therefore their proper colours, when they fall upon the paper, *v*, and so by the confusion and perfect mixture of those colours compound the Whiteness of the light reflected from thence.

Exper. 10. Let that spectrum or solar image *PT* [in *fig. 6.*] fall now upon the lens *MN*, above four inches broad, and about six feet distant from the prism *ABC*, and so figured, that it may cause the coloured light, which divergeth from the prism, to converge and meet again at its focus *G* ; about six or eight feet distant from the lens, and there to fall perpendicularly upon a white paper, *DE*. And if you move this paper to and fro, you will perceive that near the lens, as at *de*, the whole solar image, suppose at *pt*, will appear upon it intensely coloured after the manner above-explained ; and that by receding from the lens, those colours will perpetually come towards one another, and by mixing more and more dilute one another continually, until at length the paper come to the focus *G*, where by a perfect mixture they will wholly vanish, and be converted into Whiteness ; the whole light appearing now upon the paper like a little white circle. And afterwards,

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wards, by receding farther from the lens, the rays, which before converged, will now cross one another in the focus G , and diverge from thence, and thereby make the colours to appear again, but yet in a contrary order; suppose at de , where the Red r is now above which before was below, and the Violet p is below which before was above.

Let us now stop the paper at the focus G , where the light appears totally white and circular, and let us consider its Whiteness. I say, that this is composed of the converging colours. For if any of those colours be intercepted at the lens, the Whiteness will cease, and degenerate into that colour, which ariseth from the Composition of the other colours which are not intercepted. And then if the intercepted colours be let pass and fall upon that compound colour, they mix with it, and by their mixture restore the Whiteness. So if the Violet, Blue and Green be intercepted, the remaining Yellow, Orange and Red will compound upon the paper an Orange; and then if the intercepted colours be let pass, they will fall upon this compound Orange, and together with it decompound a White. So also if the Red and Violet be intercepted, the remaining Yellow, Green and Blue, will compound a Green upon the paper; and then the Red and Violet being let pass will fall upon this Green, and together with it decompound a White. And that in this composition of White the several rays do not suffer any change in their colorific qualities, by acting upon one another, but are only mixed, and by a mixture of their colours produce White, may farther appear by these Arguments.

If the paper be placed beyond the focus G , suppose at de , and then the Red colour at the lens be alternately intercepted, and let pass again, the Violet colour on the paper will not suffer any change thereby; as it ought to do, if the several sorts of rays acted upon one another in the focus G , where they cross. Neither will the Red upon the paper be changed by any alternate stopping, and letting pass the Violet which crosseth it.

And if the paper be placed at the focus G , and the white round image at G be viewed through the prism HIK , and by the refraction of that prism be translated to the place rt , and there appear tinged with various colours, namely, the Violet at v and
Red

Red at r , and others between; and then the Red colour at the lens be often stopped and let pass by turns, the Red at r will accordingly disappear and return as often, but the Violet at v will not thereby suffer any change. And so by stopping and letting pass alternately the Blue at the lens, the Blue at r will accordingly disappear and return, without any change made in the Red at r . The Red therefore depends on one sort of rays, and the Blue on another sort, which in the focus G , where they are commixed, do not act on one another. And there is the same reason of the other colours.

I considered farther, that when the most refrangible rays, rp , and the least refrangible ones, rt , are by converging inclined to one another; the paper, if held very oblique to those rays in the focus, G , might reflect one sort of them more copiously than the other sort; and by that means the reflected light would be tinged in that focus with the colour of the predominant rays; provided those rays severally retained their colours or colorific qualities, in the composition of White made by them in that focus. But if they did not retain them in that White, but became all of them severally endued there with a disposition to strike the sense with the perception of White, then they could never lose their Whiteness by such reflexions. I inclined therefore the paper to the rays very obliquely, as in the second Experiment of this Book, that the most refrangible rays might be more copiously reflected than the rest, and the Whiteness at length changed successively into Blue, Indigo and Violet. Then I inclined it the contrary way, that the least refrangible rays might be more copious in the reflected light than the rest, and the Whiteness turned successively to Yellow, Orange and Red.

Lastly, I made an instrument xy in fashion of a comb, whose teeth, being in number sixteen, were about an inch and an half broad, and the intervals of the teeth about two inches wide. Then by interposing successively the teeth of this instrument near the lens, I intercepted part of the colours by the interposed tooth, whilst the rest of them went on through the interval of the teeth to the paper de , and there painted a round Solar image. But the paper I had first placed so, that the image might appear

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White, as often as the comb was taken away; and then the comb being as was said interposed, that Whiteness, by reason of the intercepted part of the colours at the lens, did always change into the colour compounded of those colours which were not intercepted; and that colour was, by the motion of the comb, perpetually varied; so that in the passing of every tooth over the lens all these colours, Red, Yellow, Green, Blue and Purple, did always succeed one another. I caused therefore all the teeth to pass successively over the lens, and when the motion was slow, there appeared a perpetual succession of the colours upon the paper: but if I so much accelerated the motion, that the colours, by reason of their quick succession, could not be distinguished from one another, the appearance of the single colours ceased. There was no Red, no Yellow, no Green, no Blue, nor Purple to be seen any longer, but from a confusion of them all there arose one uniform White colour. Of the light which now by the mixture of all the colours appeared White, there was no part really White. One part was Red, another Yellow, a third Green, a fourth Blue, a fifth Purple, and every part retains its proper colour till it strike the sensorium. If the impressions follow one another slowly, so that they may be severally perceived, there is made a distinct sensation of all the colours one after another in a continual succession. But if the impressions follow one another so quickly that they cannot be severally perceived, there ariseth out of them all one common sensation, which is neither of this colour alone nor of that alone; but hath itself indifferently to them all, and this is a sensation of Whiteness. By the quickness of the successions, the impressions of the several colours are confounded in the sensorium, and out of that confusion ariseth a mixed sensation. If a burning coal be nimbly moved round in a circle with gyrations continually repeated, the whole circle will appear like fire; the reason of which is, that the sensation of the coal, in the several places of that circle, remains impressed on the sensorium, until the coal return again to the same place. And so in a quick consecution of the colours, the impression of every colour remains in the sensorium, until a revolution of all the colours be compleated, and that first colour return again. The impressions therefore

therefore of all the successive colours are at once in the sensorium, and jointly stir up a sensation of them all; and so it is manifest by this Experiment, that the commixed impressions of all the colours do stir up and beget a sensation of White, that is, that Whiteness is compounded of all the colours.

And if the comb be now taken away, that all the colours may at once pass from the lens to the paper, and be there intermixed, and together reflected thence to the spectators eyes; their impressions on the sensorium, being now more subtilly and perfectly commixed there, ought much more to stir up a sensation of Whiteness.

You may instead of the lens use two prisms [*fig. 7.*] *HIK* and *LMN*, which by refracting the coloured light the contrary way to that of the first refraction, may make the diverging rays converge and meet again in *G*; as you see represented in the seventh figure. For where they meet and mix they will compose a White light, as when a lens is used.

Exper. 11. Let the sun's coloured image *PT* [*in fig. 8.*] fall upon the wall of a dark chamber, as in the third Experiment of the first Book, and let the same be viewed through a prism, *abc*, held parallel to the prism, *ABC*, by whose refraction that image was made, and let it now appear lower than before, suppose in the place *s*, over against the Red colour *T*. And if you go near to the image *PT*, the spectrum *s* will appear oblong and coloured like the image *PT*; but if you recede from it, the colours of the spectrum, *s*, will be contracted more and more, and at length vanish; that spectrum *s* becoming perfectly round and white: and if you recede yet farther, the colours will emerge again, but in a contrary order. Now that spectrum *s* appears White, in that case when the rays of several sorts, which converge from the several parts of the image, *PT*, to the prism *abc*, are so refracted unequally by it, that in their passage from the prism to the eye, they may diverge from one and the same point of the spectrum *s*, and so fall afterwards upon one and the same point in the bottom of the eye, and there be mingled.

And farther, if the comb be here made use of, by whose teeth the colours at the image, *PT*, may be successively intercepted;

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the spectrum *s*, when the comb is moved slowly, will be perpetually tinged with successive colours: but when by accelerating the motion of the comb, the succession of the colours is so quick that they cannot be severally seen, that spectrum *s*, by a confused and mixed sensation of them all, will appear White.

Exper. 12. The sun shining through a large prism *ABC* [in *fig. 9.*] upon a comb *xy*, placed immediately behind the prism, his light, which passed through the interstices of the teeth, fell upon a white paper *DE*. The breadths of the teeth were equal to their interstices, and seven teeth together with their interstices took up an inch in breadth. Now when the paper was about two or three inches distant from the comb, the light, which passed through its several interstices, painted so many ranges of colours, *kl, mn, op, qr*, &c. which were parallel to one another and contiguous, and without any mixture of White. And these ranges of colours, if the comb was moved continually up and down with a reciprocal motion, ascended and descended in the paper, and when the motion of the comb was so quick, that the colours could not be distinguished from one another, the whole paper, by their confusion and mixture in the sensorium, appeared White.

Let the comb now rest; and let the paper be removed farther from the prism, and the several ranges of colours will be dilated and expanded into one another more and more, and, by mixing their colours, will dilute one another, and at length, when the distance of the paper from the comb is about a foot, or a little more (suppose in the place *2D2E*) they will so far dilute one another as to become White.

With any obstacle let all the light be now stopped, which passes through any one interval of the teeth; so that the range of colours which comes from thence may be taken away: and you will see the light of the rest of the ranges to be expanded into the place of the range taken away, and there to be coloured. Let the intercepted range pass on as before, and its colours, falling upon the colours of the other ranges, and mixing with them, will restore the Whiteness.

Let the paper *2D2E* be now very much inclined to the rays, so that the most refrangible rays may be more copiously reflected than

than the rest; and the White colour of the paper, through the excess of those rays, will be changed into Blue and Violet. Let the paper be as much inclined the contrary way, that the least refrangible rays may be now more copiously reflected than the rest, and by their excess the Whiteness will be changed into Yellow and Red. The several rays therefore in that White light do retain their colorific qualities, by which those of any sort, whenever they become more copious than the rest, do by their excess and predominance cause their proper colour to appear.

And by the same way of arguing, applied to the third Experiment of this Book, it may be concluded, that the White colour of all refracted light, at its very first emergence, where it appears as white as before its incidence, is compounded of various colours.

Exper. 13. In the foregoing Experiment the several Intervals of the teeth of the comb do the office of so many prisms, every interval producing the phenomenon of one prism. Whence instead of those intervals using several prisms, I tried to compound Whiteness by mixing their colours; and did it by using only three prisms, as also by using only two as before. Let two prisms, *ABC* and *abc* [in *fig. 10.*] whose refracting angles, *B* and *b*, are equal, be so placed parallel to one another, that the refracting angle *B* of the one may touch the angle *c* at the base of the other, and their planes, *CB* and *cb*, at which the rays emerge, may lie *in directum*. Then let the light trajected through them fall upon the paper *MN*, distant about 8 or 12 inches from the prisms. And the colours generated by the interior limits, *B* and *c*, of the two prisms, will be mingled at *PT*, and there compound White. For if either prism be taken away, the colours made by the other will appear in that place *PT*; and when the prism is restored to its place again, so that its colours may there fall upon the colours of the other, the mixture of them both will restore the Whiteness.

This Experiment succeeds also, as I have tried, when the angle *b* of the lower prism is a little greater than the angle *B* of the upper, and between the interior angles *B* and *c*, there intercedes some space *BC*, as is represented in the figure, and the refracting planes, *BC* and *bc*, are neither *in directum* nor parallel to one another.

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other. For there is nothing more requisite to the success of this Experiment, than that the rays of all sorts may be uniformly mixed upon the paper, in the place *PT*. If the most refrangible rays, coming from the superior prism, take up all the space from *M* to *P*; the rays of the same sort, which come from the inferior prism, ought to begin at *P*, and take up all the rest of the space from thence towards *N*. If the least refrangible rays, coming from the superior prism, take up the space *MT*; the rays of the same kind, which come from the other prism, ought to begin at *T*, and take up the remaining space *TN*. If one sort of the rays which have intermediate degrees of refrangibility, and come from the superior prism, be extended through the space *MQ*, and another sort of those rays through the space *MR*, and a third sort of them through the space *MS*; the same sorts of rays, coming from the lower prism, ought to illuminate the remaining spaces *QN*, *RN*, *SN*, respectively. And the same is to be understood of all the other sorts of rays. For thus the rays of every sort will be scattered uniformly and evenly through the whole space *MN*, and so being every where mixed in the same proportion, they must every where produce the same colours. And therefore since by this mixture they produce White in the exterior spaces *MP* and *TN*, they must also produce White in the interior space *PT*. This is the reason of the composition by which Whiteness was produced in this Experiment; and by what other way soever I made the like Composition, the result was Whiteness.

Lastly, if with the teeth of a comb of a due size, the coloured lights of the two prisms, which fall upon the space *PT*, be alternately intercepted; that space *PT*, when the motion of the comb is slow, will always appear coloured. But by accelerating the motion of the comb so much, that the successive colours cannot be distinguished from one another, it will appear White.

Exper. 14. Hitherto I have produced Whiteness by mixing the colours of prisms. If now the colours of Natural bodies are to be mingled, let water a little thickened with soap be agitated to raise a froth; and after that froth has stood a little, there will appear to one, that shall view it intently, various colours every where in the surfaces of the several bubbles; but to one that shall

shall go so far off that he cannot distinguish the colours from one another, the whole froth will grow White with a perfect whiteness.

Exper. 15. Lastly, in attempting to compound a White by mixing the coloured powders, which painters use; I considered that all coloured powders do suppress and stop in them a very considerable part of the light, by which they are illuminated. For they become coloured by reflecting the light of their own colours more copiously, and that of all other colours more sparingly; and yet they do not reflect the light of their own colours so copiously as White bodies do. If red lead, for instance, and a white paper, be placed in the red light of the coloured spectrum, made in a dark chamber by the refraction of a prism, as is described in the third Experiment of the first Book; the paper will appear more lucid than the red lead, and therefore reflects the Red-making rays more copiously than red lead doth. And if they be held in the light of any other colour, the light reflected by the paper will exceed the light reflected by the red lead in a much greater proportion. And the like happens in powders of other colours. And therefore by mixing such powders we are not to expect a strong and full White, such as is that of paper, but some dusky obscure one, such as might arise from a mixture of light and darkness, or from White and Black; that is, a Grey, or Dun, or Russet-brown, such as are the colours of a man's nail, of a mouse, of ashes, of ordinary stones, of mortar, of dust and dirt in highways, and the like. And such a dark White I have often produced by mixing coloured powders. For thus one part of red lead, and five parts of *Viride Aeris*, composed a dun colour like that of a mouse. For these two colours were severally so compounded of others, that in both together were mixtures of all colours; and there were less red lead used than *Viride Aeris*, because of the fullness of its colour. Again, one part of red lead, and four parts of blue bise, composed a dun colour verging a little to Purple; and by adding to this a certain mixture of Orpiment and *Viride Aeris* in a due proportion, the mixture lost its purple tincture, and became perfectly Dun. But the Experiment succeeded best without Minium thus. To Orpiment I added

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added by little and little a certain full bright Purple, which painters use, until the orpiment ceased to be Yellow, and became of a pale Red. Then I diluted that Red by adding a little *Viride Æris*, and a little more blue bise than *Viride Æris*, until it became of such a grey or pale White, as verged to no one of the colours more than to another. For thus it became of a colour equal in whiteness to that of ashes, or of wood newly cut, or of a man's skin. The orpiment reflected more light than did any other of the powders, and therefore conduced more to the Whiteness of the compounded colour than they. To assign the proportions accurately may be difficult, by reason of the different goodness of powders of the same kind. Accordingly as the colour of any powder is more or less full and luminous, it ought to be used in a less or greater proportion.

Now considering that these grey and dun colours may be also produced by mixing Whites and Blacks, and by consequence differ from perfect Whites not in species of colours, but only in degree of luminousness, it is manifest that there is nothing more requisite to make them perfectly white than to increase their light sufficiently; and, on the contrary, if by increasing their light they can be brought to perfect Whiteness, it will thence also follow, that they are of the same species of colour with the best Whites, and differ from them only in the quantity of light. And this I tried as follows. I took the third of the above-mentioned grey mixtures (that which was compounded of Orpiment, Purple, Bise and *Viride Æris*) and rubbed it thickly upon the floor of my chamber, where the sun shone upon it through the opened casement; and by it, in the shadow, I laid a piece of white paper of the same bigness. Then going from them to the distance of 12 or 18 feet, so that I could not discern the unevenness of the surface of the powder, nor the little shadows let fall from the gritty particles thereof; the powder appeared intensely White, so as to transcend even the paper itself in Whiteness, especially if the paper were a little shaded from the light of the clouds; and then the paper compared with the powder appeared of such a grey colour as the powder had done before. But by laying the paper where the sun shines through the glass of the window, or by shutting

shutting the window, that the sun might shine through the glass upon the powder, and by such other fit means of increasing or decreasing the lights, wherewith the powder and paper were illuminated; the light wherewith the powder is illuminated may be made stronger, in such a due proportion, than the light wherewith the paper is illuminated, that they shall both appear exactly alike in Whiteness. For when I was trying this, a friend coming to visit me, I stopped him at the door; and before I told him what the colours were, or what I was doing, I asked him, Which of the two Whites were the best, and wherein they differed? And after he had at that distance viewed them well, he answered, That they were both good Whites, and that he could not say which was best, nor wherein their colours differed. Now if you consider, that this White of the powder in the sun-shine was compounded of the colours which the component powders, Orpiment, Purple, Bise, and *Viride Æris*, have in the same sun-shine; you must acknowledge by this Experiment, as well as by the former, that perfect Whiteness may be compounded of colours.

From what has been said it is also evident, that the Whiteness of the sun's light is compounded of all the colours wherewith the several sorts of rays, whereof that light consists, when by their several refrangibilities they are separated from one another, do tinge paper or any other white body whereon they fall. For those colours, by Prop. II. are unchangeable, and whenever all those rays, with those their colours, are mixed again, they reproduce the same White light as before.

P R O P. VI. P R O B. II.

In a mixture of Primary Colours, the quantity and quality of each being given, to know the colour of the compound.

With the center o [in fig. 11.] and radius od describe a circle ADF; and distinguish its circumference into seven parts, DE, EF, FG, GA, AB, BC, CD, proportional to the seven musical tones or intervals of the eight sounds, *sol, la, fa, sol, la, mi, fa, sol*, contained in an eight; that is, proportional to the numbers $\frac{1}{9}, \frac{1}{10}, \frac{1}{10}, \frac{1}{9}, \frac{1}{10}, \frac{1}{10}, \frac{1}{9}$. Let the first part, DE, represent a Red colour; the second,

To ascertain
the Colour
of a

cond, EF, Orange; the third, FG, Yellow; the fourth, GA, Green; the fifth, AB, Blue; the sixth, BC, Indigo; and the seventh, CD, Violet. And conceive that these are all the colours of uncompounded light gradually passing into one another, as they do when made by prisms; the circumference DEFGABCD, representing the whole series of colours from one end of the sun's coloured image to the other; so that from D to E be all degrees of Red; at E, the mean colour between Red and Orange; from E to F, all degrees of Orange; at F, the mean between Orange and Yellow; from F to G, all degrees of Yellow, and so on. Let p be the center of gravity of the arch DE, and q, r, s, t, u, x , the centers of gravity of the arches EF, FG, GA, AB, BC and CD respectively; and about those centers of gravity let circles, proportional to the number of rays of each colour in the given mixture, be described; that is, the circle p proportional to the number of the Red-making rays in the mixture; the circle q proportional to the number of the Orange-making rays in the mixture, and so of the rest. Find the common center of gravity of all those circles p, q, r, s, t, u, x . Let that center be z ; and from the center of the circle ADF, through z to the circumference, drawing the right line oy , the place of the point y in the circumference shall shew the colour arising from the composition of all the colours in the given mixture; and the line oz shall be proportional to the fulness or intenseness of the colour, that is, to its distance from Whiteness. As if y fall in the middle between F and G, the compounded colour shall be the best Yellow; if y verge from the middle towards F or G, the compound colour shall accordingly be a Yellow, verging towards Orange or Green. If z fall upon the circumference, the colour shall be intense and florid in the highest degree; if it fall in the mid-way between the circumference and center, it shall be but half so intense; that is, it shall be such a colour as would be made by diluting the intensest Yellow with an equal quantity of Whiteness; and if it fall upon the center o , the colour shall have lost all its intenseness, and become a White. But it is to be noted, that if the point z fall in or near the line ob , the main ingredients being the Red and Violet, the colour compounded shall not be any of the Prismatic colours, but a Purple, inclining to

to Red or Violet, accordingly as the point z lieth on the side of the line bo towards E or towards C; and in general the compounded Violet is more bright and more fiery than the uncompounded. Also if only two of the Primary colours, which in the circle are opposite to one another, be mixed in an equal proportion, the point z shall fall upon the center o , and yet the Colour compounded of those two shall not be perfectly White, but some faint anonymous colour. For I could never yet by mixing only two Primary colours produce a perfect White. Whether it may be compounded of a mixture of three, taken at equal distances in the circumferences, I do not know; but of four or five I do not much question but it may. But these are curiosities of little or no moment to the understanding the phænomena of Nature. For in all Whites, produced by Nature, there uses to be a mixture of all sorts of rays, and by consequence a composition of all colours.

To give an instance of this rule; suppose a colour is compounded of these Homogeneous colours, of Violet one part, of Indigo one part, of Blue two parts, of Green three parts, of Yellow five parts, of Orange six parts, and of Red ten parts. Proportional to these parts describe the circles x, v, t, s, r, q, p , respectively; that is, so that if the circle x be one, the circle v may be one, the circle t two, the circle s three, and the circles r, q and p , five, six and ten. Then I find z the common center of gravity of these circles; and through z drawing the line oy , the point y falls upon the circumference between E and F, something nearer to E than to F; and thence I conclude, that the colour compounded of these ingredients will be an Orange, verging a little more to Red than to Yellow. Also I find that oz is a little less than one half of oy ; and thence I conclude, that this Orange hath a little less than half the fulness or intenseness of an uncompounded Orange; that is to say, that it is such an Orange, as may be made by mixing an Homogeneous Orange with a good White, in the proportion of the line oz to the line zy ; this proportion being not of the quantities of mixed orange and white powders, but of the quantities of the lights reflected from them.

This Rule I conceive accurate enough for practice, though not mathematically accurate; and the truth of it may be sufficiently proved

proved to sense, by stopping any of the colours at the lens in the tenth Experiment of this Book. For the rest of the colours, which are not stopped, but pass on to the focus of the lens, will there compound, either accurately or very nearly, such a colour, as by this Rule ought to result from their mixture.

P R O P. VII. T H E O R. V.

All the colours in the universe which are made by light, and depend not on the power of imagination, are either the colours of Homogeneous Lights, or compounded of these; and that either accurately or very nearly, according to the Rule of the foregoing Problem.

All Colours
either

For it has been proved (in Prop. I. Part II.) that the changes of colours, made by refractions, do not arise from any new modifications of the rays impressed by those refractions, and by the various terminations of light and shadow, as has been the constant and general opinion of philosophers. It has also been proved, that the several colours of the Homogeneous Rays do constantly answer to their degrees of refrangibility (Prop. I. Part I. and Prop. II. Part II.) and that their degrees of refrangibility cannot be changed by refractions and reflexions (Prop. II. Part I.) and by consequence that those their colours are likewise immutable. It has also been proved directly, by refracting and reflecting Homogeneous Lights apart, that their colours cannot be changed (Prop. II. Part II.) It has been proved also, that when the several sorts of rays are mixed, and in crossing pass through the same space, they do not act on one another so as to change each others colorific qualities (Exper. 10. Part II.) but, by mixing their actions in the sensorium, beget a sensation differing from what either would do apart, that is a sensation of a mean colour between their proper colours; and particularly when by the concurrence and mixtures of all sorts of rays, a White colour is produced; the White is a mixture of all the colours, which the rays would have apart (Prop. v. Part II.) The rays in that mixture do not lose or alter their several colorific qualities, but by all their various kinds of actions, mixed in the sensorium, beget a sensation of a middling colour between

between all their colours, which is Whiteness. For Whiteness is a mean between all colours, having itself indifferently to them all, so as with equal facility to be tinged with any of them. A Red powder mixed with a little Blue, or a Blue with a little Red, doth not presently lose its colour; but a White powder mixed with any colour is presently tinged with that colour, and is equally capable of being tinged with any colour whatever. It has been shewed also, that as the sun's light is mixed of all sorts of rays, so its Whiteness is a mixture of the colours of all sorts of rays; those rays having from the beginning their several colorific qualities, as well as their several refrangibilities, and retaining them perpetually unchanged, notwithstanding any refractions or reflexions they may at any time suffer; and that whenever any sort of the sun's rays is by any means (as by reflexion in Exper. 9 and 10. Part I. or by refraction as happens in all refractions) separated from the rest, then they manifest their proper colours. These things have been proved: and the sum of all this amounts to the Proposition here to be proved. For if the sun's light is mixed of several sorts of rays, each of which have originally their several refrangibilities and colorific qualities, and notwithstanding their refractions and reflexions, and their various separations or mixtures, keep those their original properties perpetually the same without alteration; then all the colours in the world must be such, as constantly ought to arise from the original colorific qualities of the rays, whereof the lights consist, by which those colours are seen. And therefore if the reason of any colour whatever be required, we have nothing else to do, than to consider how the rays in the sun's light have, by reflexions or refractions, or other causes, been parted from one another, or mixed together; or otherwise to find out what sorts of rays are in the light, by which that colour is made, and in what proportion; and then, by the last Problem, to learn the colour which ought to arise by mixing those rays, or their colours, in that proportion. I speak here of colours so far as they arise from light. For they appear sometimes by other causes; as when by the power of phantasy we see colours in a dream; or a mad-man sees things before him which are not there; or when we see fire

by striking the eye; or see colours like the eye of a peacock's feather, by pressing our eyes in either corner, whilst we look the other way. Where these and such like causes interpose not, the colour always answers to the sort or sorts of the rays, whereof the light consists; as I have constantly found in whatever phenomena of colours I have hitherto been able to examine. I shall in the following Propositions give instances of this in the phenomena of chiefest note.

P R O P. VIII. P R O B. III.

By the discovered properties of light to explain the colours made by prisms.

The Prismatic Let ABC [in *fig. 12.*] represent a prism refracting the light of the sun, which comes into a dark chamber, through a hole, $F\phi$, almost as broad as the prism; and let MN represent a white paper, on which the refracted light is cast; and suppose the most refrangible, or deepest Violet-making, rays fall upon the space $p\pi$; the least Refrangible or deepest Red-making rays, upon the space $\tau\gamma$; the middle sort between the Indigo-making and Blue-making rays, upon the space $Q\chi$; the middle sort of the Green-making rays upon the space $R\phi$; the middle sort between the Yellow-making and Orange-making rays, upon the space $s\sigma$; and other intermediate sorts, upon intermediate spaces. For so the spaces, upon which the several sorts adequately fall, will, by reason of the different refrangibility of those sorts, be one lower than another. Now if the paper, MN, be so near the prism, that the spaces $p\tau$ and $\pi\gamma$ do not interfere with one another; the distance between them, $\tau\pi$, will be illuminated by all the sorts of rays, in that proportion to one another which they have at their very first coming out of the prism, and consequently be White. But the spaces $p\tau$ and $\pi\gamma$ on either hand, will not be illuminated by them all, and therefore will appear coloured. And particularly at p , where the outmost Violet-making rays fall alone, the colour must be the deepest Violet. At Q , where the Violet-making and Indigo-making rays are mixed, it must be a Violet inclining much to Indigo. At R , where the Violet-making, Indigo-making, Blue-

making,

making, and one half of the Green-making rays are mixed, their colours must (by the construction of the second Problem) compound a middle colour between Indigo and Blue. At s , where all the rays are mixed except the Red-making and Orange-making, their colours ought by the same rule to compound a faint Blue, verging more to Green than Indigo. And in the progress from s to τ , this Blue will grow more and more faint and dilute, till at τ , where all the colours begin to be mixed, it ends in Whiteness.

So again, on the other side of the White, at τ , where the least refrangible or outmost Red-making rays are alone, the colour must be the deepest Red. At σ , the mixture of Red and Orange will compound a Red inclining to Orange. At ϕ , the mixture of Red, Orange, Yellow, and one half of the Green must compound a middle colour between Orange and Yellow. At χ , the mixture of all colours but Violet and Indigo will compound a faint Yellow, verging more to Green than to Orange. And this Yellow will grow more faint and dilute continually in its progress from χ to π , where by a mixture of all sorts of rays it will become White.

These colours ought to appear, were the sun's light perfectly White: but because it inclines to Yellow, the excess of the Yellow-making rays, whereby it is tinged with that colour, being mixed with the faint Blue between s and τ , will draw it to a faint Green. And so the colours in order from p to τ ought to be Violet, Indigo, Blue, very faint Green, White, faint Yellow, Orange, Red. Thus it is by the computation: and they that please to view the colours made by a prism, will find it so in Nature.

These are the colours on both sides the White, when the paper is held between the prism and the point x , where the colours meet and the interjacent white vanishes. For if the paper be held still farther off from the prism, the most refrangible and least refrangible rays will be wanting in the middle of the light, and the rest of the rays which are found there, will by mixture produce a fuller Green than before. Also the Yellow and Blue will now become less compounded; and by consequence more intense than before. And this also agrees with experience.

And!

ThePrismatic

And if one look through a prism upon a white object encompassed with blackness or darkness, the reason of the colours arising on the edges is much the same; as will appear to one that shall a little consider it. If a black object be encompassed with a white one, the colours which appear through the prism are to be derived from the light of the white one, spreading into the regions of the black; and therefore they appear in a contrary order to that, when a white object is surrounded with black. And the same is to be understood when an object is viewed, whose parts are some of them less luminous than others. For in the borders of the more and less luminous parts, colours ought always, by the same principles, to arise from the excess of the light of the more luminous, and to be of the same kind as if the darker parts were black, but yet to be more faint and dilute.

What is said of colours made by prisms may be easily applied to colours made by the glasses of telescopes or microscopes, or by the humours of the eye. For if the object-glass of a telescope be thicker on one side than on the other, or if one half of the glass, or one half of the pupil of the eye be covered with any opaque substance: the object-glass, or that part of it, or of the eye, which is not covered, may be considered as a wedge with crooked sides; and every wedge of glass, or other pellucid substance, has the effect of a prism in refracting the light which passes through it (S).

How the colours in the ninth and tenth Experiments of the first Part arise from the different reflexibility of light, is evident by what was there said. But it is observable in the ninth Experiment, that whilst the sun's direct light is Yellow, the excess of the Blue making rays in the reflected beam of light MN, suffices only to bring that Yellow to a pale White inclining to Blue, and not to tinge it with a manifestly Blue colour. To obtain therefore a better Blue, I used instead of the Yellow light of the sun the White light of the clouds, by varying a little the Experiment, as follows.

Exper. 16. Let HFG [in *fig. 13.*] represent a prism in the open air; and s, the eye of the spectator, viewing the clouds by their light coming into the prism at the plane side FIGK, and reflected

(1) Vide Lect. Opt. Part II. Sect. v.

in

in it by its base HEIG, and thence going out through its plane side HEFK to the eye. And when the prism and eye are conveniently placed, so that the angles of incidence and reflexion at the base may be about 40 degrees; the spectator will see a bow MN of a Blue colour, running from one end of the base to the other, with the concave side towards him; and the part of the base IMNG beyond this bow will be brighter than the other part, EMNH, on the other side of it. This Blue colour MN being made by nothing else than by Reflexion of a specular superficies, seems so odd a phenomenon, and so difficult to be explained by the vulgar hypothesis of philosophers, that I could not but think it deserved to be taken notice of. Now for understanding the reason of it, suppose the plane ABC to cut the plane sides and base of the prism perpendicularly. From the eye to the line BC, wherein that plane cuts the base, draw the lines sp and st, in the angles $s\hat{p}c$ 50 degr. $\frac{1}{3}$, and $s\hat{t}c$ 49 degr. $\frac{1}{3}$: and the point p will be the limit, beyond which none of the most refrangible rays can pass through the base of the prism, and be refracted, whose incidence is such that they may be reflected to the eye; and the point t will be the like limit for the least refrangible rays, that is, beyond which none of them can pass through the base, whose incidence is such that by reflexion they may come to the eye. And the point r taken in the middle way between p and t, will be the like limit for the meanly refrangible rays. And therefore all the least refrangible rays which fall upon the base beyond t, that is, between t and B, and can come from thence to the eye, will be reflected thither: but on this side t, that is, between t and c, many of these rays will be transmitted through the base. And all the most refrangible rays which fall upon the base beyond p, that is, between p and B, and can by reflexion come from thence to the eye, will be reflected thither; but every where between p and c, many of these rays will get through the base and be refracted: and the same is to be understood of the meanly refrangible rays on either side of the point r. Whence it follows, that the base of the prism must every where between t and B, by a total reflexion of all sorts of rays to the eye, look white and bright. And every where between p and c, by reason of the transmission

sion of many rays of every sort, look more pale, obscure and dark. But at r , and in other places between p and t , where all the more refrangible rays are reflected to the eye, and many of the less refrangible are transmitted, the excess of the most refrangible in the Reflected Light will tinge that light with their colour, which is Violet and Blue. And this happens by taking the line $cpri$ any where between the ends of the prism, nc and ei ^(b).

P R O P. IX. P R O B. IV.

By the discovered Properties of Light to explain the Colours of the Rainbow.

The
RAINBOW.

This bow never appears, but where it rains in the sun-shine, and may be made artificially by spouting up water which may break aloft, and scatter into drops, and fall down like rain. For the sun shining upon these drops certainly causes the bow to appear to a spectator standing in a due position to the rain and sun. And hence it is now agreed upon, that this bow is made by refraction of the sun's light in drops of falling rain. This was understood by some of the ancients, and of late more fully discovered and explained by the famous *Antonius de Dominis* archbishop of *Spalato*, in his book *De Radiis Visis & Lucis*, published by his friend *Bartolus* at *Venice*, in the year 1611, and written above 20 years before. For he teaches there how the interior bow is made in round drops of rain by two refractions of the sun's light, and one reflexion between them; and the exterior, by two refractions and two sorts of reflexions between them in each drop of water; and proves his explications by Experiments made with a phial full of water, and with globes of glass filled with water, and placed in the sun to make the colours of the two bows appear in them. The same explication *Des-Cartes* hath pursued in his *Meteors*, and mended that of the exterior bow. But whilst

^(b) Vid. Lect. Opt. Part II. § 145—149.

^(c) Vid. Lect. Opt. Part I. Sect. IV. Prop. xxxv. Cor. 1 & 2.

^(d) Lect. Opt. Part I. Sect. IV. Prop. xxxvi. Cor. 1 & 2.

they

they understood not the true origin of colours, it is necessary to ^{The} pursue it here a little farther. For understanding therefore how ^{RAINBOW.} the bow is made, let a drop of rain, or any other spherical transparent body, be represented by the sphere $BNFG$ [in *fig. 14.*] described with the center C , and semi-diameter CN . And let AN be one of the sun's rays, incident upon it at N , and thence refracted to F ; where let it either go out of the sphere by refraction towards V , or be reflected to G ; and at G , let it either go out by refraction to R , or be reflected to H ; and at H , let it go out by refraction towards S , cutting the incident ray in Y . Produce AN and RG till they meet in X ; and upon AX and NF let fall the perpendiculars CD and CE ; and produce CD , till it fall upon the circumference at L . Parallel to the incident ray, AN , draw the diameter BQ ; and let the sine of incidence out of air into water be to the sine of refraction as I to R . Now if you suppose the point of incidence, N , to move from the point B continually, till it come to L , the arch QF will first increase and then decrease; and so will the angle AXR , which the rays AN and GR contain; and the arch QF , and angle AXR , will be biggest, when ND is to CN as $\sqrt{11-RR}$ to $\sqrt{3RR}$; in which case NE will be to ND as $2R$ to I ⁽ⁱ⁾. Also the angle AYS , which the rays AN and HS contain, will first decrease, and then increase; and grow least, when ND is to CN as $\sqrt{11-RR}$ to $\sqrt{8RR}$; in which case NE will be to ND as $3R$ to I ^(k). And so the angle, which the next emergent ray (that is, the emergent ray after three reflexions) contains with the incident ray AN , will come to its limit, when ND is to CN as $\sqrt{11-RR}$ to $\sqrt{15RR}$; in which case NE will be to ND as $4R$ to I . And the angle, which the ray next after that emergent, that is, the ray emergent after four reflexions, contains with the incident, will come to its limit, when ND is to CN as $\sqrt{11-RR}$ to $\sqrt{24RR}$; in which case NE will be to ND as $5R$ to I : and so on infinitely, the numbers 3, 8, 15, 24, &c. being gathered by continual addition of the terms of the Arithmetical Progression 3, 5, 7, 9, &c. The truth of all this Mathematicians will easily examine ^(l).

⁽ⁱ⁾ It all follows very evidently from the demonstrations our author hath given of the two Propositions referred to in Note ^a and ^b.

The
RAINBOW.

Now it is to be observed, that as when the sun comes to his tropicks, days increase and decrease but a very little for a great while together; so when by increasing the distance CD , these angles come to their limits, they vary their quantity but very little for some time together; and therefore a far greater number of the rays, which fall upon all the points N in the quadrant BL , shall emerge in the limits of these angles, than in any other inclinations. And farther it is to be observed, that the rays, which differ in refrangibility, will have different limits of their angles of emergence; and, by consequence, according to their different degrees of refrangibility, emerge most copiously in different angles, and being separated from one another appear each in their proper colours. And what those angles are, may be easily gathered from the foregoing Theorem by computation.

For in the least refrangible rays the sines I and R (as was found above) are 108 and 81, and thence by computation the greatest angle AXR will be found 42 degrees and 2 minutes; and the least angle AYS 50 degrees and 57 minutes. And in the most refrangible rays the sines I and R are 109 and 81; and thence by computation the greatest angle AXR will be found 40 degrees and 17 minutes, and the least angle AYS 54 degrees and 7 minutes.

Suppose now that O [in *fig. 15.*] is the spectator's eye, and OP a line drawn parallel to the sun's rays; and let POE , POF , POG , POH , be angles of 40 degr. 17 min. 42 degr. 2 min. 50 degr. 57 min. and 54 degr. 7 min. respectively; and these angles turned about their common side OP , shall with their other sides, OE , OF , OG , OH , describe the verges of two rain-bows $AFBE$ and $CHDG$. For if E , F , G , H be drops, placed any where in the conical superficies described by OE , OF , OG , OH , and be illuminated by the sun's rays SE , SF , SG , SH ; the angle SEO being equal to the angle POE or 40 deg 17 min. shall be the greatest angle, in which the most refrangible rays can, after one reflexion, be refracted to the eye; and therefore all the drops in the line OE shall send the most refrangible rays most copiously to the eye, and thereby strike the senses with the deepest Violet colour in that region. And in like manner the angle SFO being equal to the angle POF , or 42 degr. 2 min. shall be the greatest, in which the least refrangible rays,

after

after one reflexion, can emerge out of the drops; and therefore ^{The} those rays shall come most copiously to the eye from the drops in the line OF , and strike the senses with the deepest Red colour in that region. And by the same argument, the rays, which have intermediate degrees of refrangibility, shall come most copiously from drops between E and F , and strike the senses with the intermediate colours, in the order which their degrees of refrangibility require; that is in the progress from E to F , or from the inside of the bow to the outside, in this order; Violet, Indigo, Blue, Green, Yellow, Orange, Red. But the Violet, by the mixture of the White light of the clouds, will appear faint, and incline to Purple.

Again, the angle SGO being equal to the angle POG , or 50 gr. 51 min. shall be the least angle, in which the least refrangible rays can, after two reflexions, emerge out of the drops; and therefore the least refrangible rays shall come most copiously to the eye from the drops in the line OG , and strike the sense with the deepest Red in that region. And the angle SHO being equal to the angle POH , or 54 gr. 7 min. shall be the least angle, in which the most refrangible rays, after two reflexions, can emerge out of the drops; and therefore those rays shall come most copiously to the eye from the drops in the line OH , and strike the senses with the deepest Violet in that region. And by the same argument, the drops in the regions between G and H shall strike the sense with the intermediate colours, in the order which their degrees of refrangibility require; that is, in the progress from G to H , or from the inside of the bow to the outside, in this order; Red, Orange, Yellow, Green, Blue, Indigo, Violet. And since these four lines OE , OF , OG , OH , may be situated any where in the abovementioned Conical Superficies; what is said of the drops and colours in these lines is to be understood of the drops and colours every where in those superficies.

Thus shall there be made two bows of colours, an interior and stronger, by one reflexion in the drops, and an exterior and fainter by two; for the light becomes fainter by every reflexion. And their colours shall lie in a contrary order to one another, the Red of both bows bordering upon the space, GF , which is between the

the

The
RAINBOW.

the bows. The breadth of the interior bow, EOF, measured cross the colours, shall be 1 deg. 45 min. and the breadth of the exterior, GOH, shall be 3 deg. 10 min. and the distance between them GOF shall be 8 gr. 55 min. the greatest semi-diameter of the innermost, that is, the angle EOF being 42 gr. 2 min. and the least semi-diameter of the outermost FOG, being 50 gr. 57 min. These are the measures of the bows, as they would be, were the sun but a point; for by the breadth of his body the breadth of the bows will be increased and their distance decreased by half a degree, and so the breadth of the interior Iris will be 2 deg. 15 min. that of the exterior 3 deg. 40 min. their distance 8 deg. 25 min. the greatest semi-diameter of the interior bow 42 deg. 17 min. and the least of the exterior 50 deg. 42 min. And such are the dimensions of the bows in the heavens found to be very nearly, when their colours appear strong and perfect. For once, by such means as I then had, I measured the greatest semi-diameter of the interior Iris about 42 degrees; and the breadth of the Red, Yellow and Green in that Iris, 63 or 64 minutes; besides the outmost faint Red obscured by the brightness of the clouds, for which we may allow 3 or 4 minutes more. The breadth of the Blue was about 40 minutes more besides the Violet, which was so much obscured by the brightness of the clouds, that I could not measure its breadth. But supposing the breadth of the Blue and Violet together to equal that of the Red, Yellow and Green together, the whole breadth of this iris will be about $2\frac{1}{4}$ degrees, as above. The least distance between this iris and the exterior iris was about 8 degrees and 30 minutes. The exterior iris was broader than the interior, but so faint, especially on the Blue side, that I could not measure its breadth distinctly. At another time when both bows appeared more distinct, I measured the breadth of the interior iris 2 gr. 10, and the breadth of the Red, Yellow and Green in the exterior iris, was to the breadth of the same colours in the interior as 3 to 2.

This explication of the rain-bow is yet farther confirmed by the known experiment, made by *Antonius de Dominis* and *Descartes*, of hanging up any where in the sun-shine a glass globe filled with water; and viewing it in such a posture, that the rays,

which come from the globe to the eye, may contain with the sun's rays an angle of either 42 or 50 degrees. For if the angle be about 42 or 43 degrees, the spectator, suppose at o, shall see a full Red colour in that side of the globe opposed to the sun, as it is represented at F; and if that angle become less, suppose by depressing the globe to E, there will appear other colours, Yellow, Green and Blue successively in the same side of the globe. But if the angle be made about 50 degrees, suppose by lifting up the globe to G, there will appear a Red colour in that side of the globe towards the sun; and if the angle be made greater, suppose by lifting up the globe to H, the Red will turn successively to the other colours, Yellow, Green and Blue. The same thing I have tried by letting a globe rest, and raising or depressing the eye, or otherwise moving it to make the angle of a just magnitude.

I have heard it represented, that if the light of a candle be refracted by a prism to the eye; when the Blue colour falls upon the eye, the spectator shall see Red in the prism, and when the Red falls upon the eye he shall see Blue: and if this were certain, the colours of the globe and rain-bow ought to appear in a contrary order to what we find. But the colours of the candle being very faint, the mistake seems to arise from the difficulty of discerning what colours fall on the eye. For, on the contrary, I have sometimes had occasion to observe in the sun's light, refracted by a prism, that the spectator always sees that colour in the prism, which falls upon his eye. And the same I have found true also in candle-light. For when the prism is moved slowly from the line which is drawn directly from the candle to the eye, the Red appears first in the prism, and then the Blue; and therefore each of them is seen when it falls upon the eye. For the Red passes over the eye first, and then the Blue.

The light which comes through drops of rain by two refractions without any reflexion, ought to appear strongest at the distance of about 26 degrees from the sun, and to decay gradually both ways, as the distance from him increases and decreases. And the same is to be understood of light transmitted through spherical hail-stones. And if the hail be a little flatted, as it often is,

HALOS and
PARHELIA.

HALOS and
PARHELIA.

is, the light transmitted may grow so strong at a little less distance than that of 26 degrees, as to form a halo about the sun or moon; which halo, as often as the hail-stones are duly figured, may be coloured. And then it must be Red within by the least refrangible rays, and Blue without by the most refrangible ones; especially if the hail-stones have opaque globules of snow in their center to intercept the light within the halo (as *Hugenius* has observed) and make the inside thereof more distinctly defined than it would otherwise be. For such hail-stones, though spherical, by terminating the light by the snow, may make a halo red within and colourless without, and darker in the red than without, as halos use to be. For of those rays which pass close by the snow, the rubriform will be least refracted, and so come to the eye in the directest lines.

The light which passes through a drop of rain after two refractions, and three or more reflexions, is scarce strong enough to cause a sensible bow; but in those cylinders of ice by which *Hugenius* explains the *Parbelia*, it may perhaps be sensible.

P R O P. X. P R O B. V.

By the discovered properties of Light to explain the Permanent Colours of Natural Bodies.

Origin of the
permanent
Colours

These colours arise from hence; that some natural bodies reflect some sorts of rays, others other sorts more copiously than the rest. Minium reflects the least refrangible or Red-making rays more copiously, and thence appears Red. Violets reflect the most refrangible, most copiously, and thence have their colour, and so of other bodies. Every body reflects the rays of its own colour more copiously than the rest, and from their excess and predominance in the Reflected Light has its colour.

Exper. 17. For if in the Homogeneous Lights, obtained by the solution of the Problem proposed in the fourth Proposition of the first Part of this Book, you place bodies of several colours; you will find, as I have done, that every body looks most splendid and luminous in the light of its own colour. Cinnaber in the Homogeneous Red light is most resplendent, in the Green light it is manifestly less

less resplendent, and in the Blue light still less. Indigo in the Violet-blue light is most resplendent, and its splendor is gradually diminished as it is removed thence by degrees through the Green and Yellow light to the Red. By a leak the Green light, and next that the Blue and Yellow which compound Green, are more strongly reflected than the other colours, Red and Violet, and so of the rest. But to make these experiments the more manifest, such bodies ought to be chosen as have the fullest and most vivid colours, and two of those bodies are to be compared together. Thus, for instance, if Cinnaber and Ultra-marine blue, or some other full blue, be held together in the Red homogeneous light, they will both appear Red; but the Cinnaber will appear of a strongly luminous and resplendent Red, and the Ultra-marine blue of a faint obscure and dark Red; and if they be held together in the Blue homogeneous light they will both appear Blue, but the Ultra-marine will appear of a strongly luminous and resplendent Blue, and the Cinnaber of a faint and dark Blue. Which puts it out of dispute, that the Cinnaber reflects the Red light more copiously than the Ultra-marine doth, and the Ultra-marine reflects the Blue much more copiously than the Cinnaber doth. The same experiment may be tried successfully with Red Lead and Indigo, or with any other two coloured bodies, if due allowance be made for the different strength and weakness of their colour and light.

And as the reason of the colours of Natural Bodies is evident by these experiments, so it is farther confirmed and put past dispute by the two first Experiments of the first Part; whereby it was proved in such bodies, that the reflecting lights, which differ in colours, do differ also in degrees of refrangibility. For thence it is certain, that some bodies reflect the more refrangible, others the less refrangible rays more copiously.

And that this is not only a true reason of these colours, but even the only reason, may appear farther from this consideration, that the colour of Homogeneous Light cannot be changed by the reflexion of Natural Bodies.

For if bodies by reflexion cannot in the least change the colour of any one sort of rays, they cannot appear coloured by

any other means, than by reflecting those which either are of their own colour, or which by mixture must produce it.

Origin of the
permanent
Colours

But in trying experiments of this kind, care must be had that the light be sufficiently Homogeneous. For if bodies be illuminated by the ordinary Prismatic Colours, they will appear neither of their own day-light colours, nor of the colour of the light cast on them; but of some middle colour between both, as I have found by experience. Thus red lead, for instance, illuminated with the ordinary Prismatic Green, will not appear either Red or Green, but Orange or Yellow, or between Yellow and Green, accordingly as the Green light, by which it is illuminated, is more or less compounded. For because red lead appears Red, when illuminated with White light, wherein all sorts of rays are equally mixed, and in the Green light all sorts of rays are not equally mixed; the excess of the Yellow-making, Green-making and Blue-making rays in the incident Green light, will cause those rays to abound so much in the Reflected light, as to draw the colour from Red towards their colour. And because the red lead reflects the Red-making rays most copiously in proportion to their number, and next after them the Orange-making and Yellow-making rays; these rays in the Reflected light will be more, in proportion to the light, than they were in the incident Green light, and thereby will draw the Reflected light from Green towards their colours. And therefore the Red lead will appear neither Red nor Green, but of a colour between both.

In transparently coloured liquors it is observable, that their colour uses to vary with their thickness. Thus, for instance, a Red liquor in a Conical glass held between the light and the eye, looks of a pale and dilute Yellow at the bottom, where it is thin; and a little higher, where it is thicker, grows Orange; and where it is still thicker, becomes Red; and where it is thickest, the Red is deepest and darkest^(m). For it is to be conceived that such a liquor stops the Indigo-making and Violet-making rays most easily, the Blue-making rays more difficultly, the Green-making rays still more difficultly, and the Red-making most difficultly: and that if the thickness of the liquor be only so much

^(m) Vide Lect. Opt. Part II. § 97.

as suffices to stop a competent number of the Violet-making and Indigo-making rays, without diminishing much the number of the rest, the rest must (by Prop. VI. Part I.) compound a pale Yellow. But if the liquor be so much thicker, as to stop also a great number of the Blue-making rays, and some of the Green-making, the rest must compound an Orange; and where it is so thick, as to stop also a great number of the Green-making, and a considerable number of the Yellow-making, the rest must begin to compound a Red; and this Red must grow deeper and darker as the Yellow-making and Orange-making rays are more and more stopped by increasing the thickness of the liquor, so that few rays besides the Red-making can get through.

Of this kind is an experiment lately related to me by Mr. Halley; who, in diving deep into the sea in a diving-veffel, found in a clear sun-shine day, that when he was sunk many fathoms deep into the water, the upper part of his hand, on which the sun shone directly through the water, and through a small glass-window in the vessel, appeared of a Red colour like that of a damask rose; and the water below, and the upper part of his hand, illuminated by light reflected from the water below, look Green. For thence it may be gathered, that the sea-water reflects back the Violet and Blue making rays most easily, and lets the Red-making rays pass most freely and copiously to great depths. For thereby the sun's direct light at all great depths, by reason of the predominating Red-making rays, must appear Red; and the greater the depth is, the fuller and intenser must that Red be. And at such depths as the Violet-making rays scarce penetrate unto, the Blue-making, Green-making and Yellow-making rays, being reflected from below more copiously than the Red-making ones, must compound a Green.

Now if there be two liquors of full colours, suppose a Red and a Blue, and both of them so thick as suffices to make their colours sufficiently full; though either liquor be sufficiently transparent apart, yet will you not be able to see through both together. For if only the Red-making rays pass through one liquor, and only the Blue-making through the other, no rays can pass through both. This Mr. Hook tried casually with glass-wedges filled

Origin of the
permanent
Colours

filled with Red and Blue liquors, and was surprized at the unexpected event; the reason of it being then unknown: which makes me trust the more to his Experiment, though I have not tried it myself. But he that would repeat it, must take care the liquors be of very good and full colours.

Now whilst bodies become coloured by reflecting or transmitting this or that sort of rays more copiously than the rest; it is to be conceived that they stop and stifle in themselves the rays, which they do not reflect or transmit. For if gold be foliated, and held between your eye and the light, the light looks of a greyish Blue; and therefore massy gold lets into its body the Blue-making rays, to be reflected to and fro within it, till they be stopped and stifled; whilst it reflects the Yellow-making outwards, and thereby looks Yellow. And much after the same manner that leaf-gold is Yellow by reflected, and Blue by transmitted light, and massy gold is Yellow in all positions of the eye; there are some liquors, as the tincture of *Lignum Nepriticum*, and some sorts of glass, which transmit one sort of light most copiously, and reflect another sort; and thereby look of several colours, according to the position of the eye to the light. But if these liquors or glasses were so thick and massy, that no light could get through them; I question not but they would, like all other opaque bodies, appear of one and the same colour in all positions of the eye, though this I cannot yet affirm by experience. For all coloured bodies, so far as my observation reaches, may be seen through, if made sufficiently thin; and therefore are in some measure Transparent, and differ only in degrees of transparency from tinged transparent liquors; these liquors, as well as those bodies, by a sufficient thickness becoming Opaque. A transparent body which looks of any colour by transmitted light, may also look of the same colour by reflected light; the light of that colour being reflected by the farther surface of the body, or by the air beyond it. And then the reflected colour will be diminished, and perhaps cease, by making the body very thick, and pitching it on the backside to diminish the reflexion of its farther surface, so that the light reflected from the tinging particles may predominate. In such cases, the colour of the reflected light

light will be apt to vary from that of the light transmitted. But whence it is that tinged bodies and liquors reflect some sort of rays, and intromit or transmit other sorts, shall be said in the next Book. In this Proposition I content myself to have put it past dispute, that bodies have such properties, and thence appear coloured.

PROP. XI. PROB. VI.

By mixing coloured lights to compound a beam of light, of the same colour and nature with a beam of the sun's direct light, and therein to experience the truth of the foregoing Propositions.

Let *ABCabc* [in fig. 16.] represent a prism by which the sun's light, let into a dark chamber through the hole *F*, may be refracted towards the lens *mn*, and paint upon it at *p, q, r, s* and *t*, the usual colours, Violet, Blue, Green, Yellow and Red; and let the diverging rays, by the refraction of this lens, converge again towards *x*, and there, by the mixture of all those their colours, compound a White, according to what was shewn above. Then let another prism *DEGdeg*, parallel to the former, be placed at *x*, to refract that White light upwards towards *y*. Let the Refracting Angles of the prisms, and their distances from the lens be equal; so that the rays which converged from the lens towards *x*, and, without refraction, would there have crossed and diverged again; may, by the refraction of the second prism, be reduced into parallelism, and diverge no more. For then those rays will recombine a beam of White light, *xy*. If the Refracting Angle of either prism be the bigger, that prism must be so much the nearer to the lens. You will know when the prisms and the lens are well set together, by observing if the beam of light *xy*, which comes out of the second prism, be perfectly White to the very edges of the light, and at all distances from the prism continue perfectly and totally White like a beam of the sun's light: For till this happens, the position of the prisms and lens to one another must be corrected; and then if by the help of a long beam of wood, as is represented in the figure, or by a tube, or some other such instrument made for that purpose, they be made fast

To recompose fast in that situation, you may try all the same experiments in this compounded beam of light xy , which have been made in the sun's direct light. For this compounded beam of light has the same appearance, and is endowed with all the same properties with a direct beam of the sun's light, so far as my observation reaches. And in trying experiments in this beam, you may, by stopping any of the colours p, q, r, s and t at the lens, see how the colours produced in the experiments are no other than those which the rays had at the lens, before they entered the composition of this beam: and by consequence, that they arise not from any new modifications of the light by refractions and reflexions, but from the various separations and mixtures of the rays originally endowed with their colour-making qualities.

So, for instance, having with a lens $4\frac{1}{4}$ inches broad, and two prisms on either hand $6\frac{1}{4}$ feet distant from the lens, made such a beam of compounded light: to examine the reason of the colours made by prisms, I refracted this compounded beam of light xy with another prism $HIKkk$, and thereby cast the usual Prismatick Colours, $pqrst$, upon the paper, LV , placed behind. And then by stopping any of the colours p, q, r, s, t at the lens, I found that the same colour would vanish at the paper. So if the Purple p was stopped at the lens, the Purple p upon the paper would vanish, and the rest of the colours would remain unaltered; unless perhaps the Blue, so far as some Purple, latent in it at the lens, might be separated from it by the following refractions. And so by intercepting the Green upon the lens, the Green r upon the paper would vanish, and so of the rest; which plainly shews, that as the White beam of light, xy , was compounded of several lights variously coloured at the lens, so the colours, which afterwards emerge out of it by new refractions, are no other than those of which its Whiteness was compounded. The refraction of the prism $HIKkk$ generates the colours $pqrst$ upon the paper, not by changing the colorific qualities of the rays, but by separating the rays, which had the very same colorific qualities before they entered the composition of the refracted beam of White light xy . For otherwise the rays which were of one colour at the lens might be of another upon the paper, contrary to what we find.

So

So again, to examine the reason of the colours of Natural Bodies, I placed such bodies in the beam of light xy , and found that they all appeared there of those their own colours which they have in day-light; and that those colours depend upon the rays, which had the same colours at the lens, before they entered the composition of that beam. Thus, for instance, Cinnaber illuminated by this beam appears of the same Red colour as in day-light; and if at the lens you intercept the Green-making and Blue-making rays, its redness will become more full and lively: but if you there intercept the Red-making rays, it will not any longer appear Red, but become Yellow or Green, or of some other colour, according to the sorts of rays which you do not intercept. So Gold in this light, xy , appears of the same Yellow colour as in day-light; but by intercepting at the lens a due quantity of the Yellow-making rays, it will appear White like silver, as I have tried; which shews that its Yellowness arises from the excess of the intercepted rays tinging that Whiteness with their colour, when they are let pass. So the infusion of *Lignum Nephriticum*, as I have also tried, when held in this beam of light, xy , looks Blue by the reflected part of the light, and Red by the transmitted part of it, as when it is viewed in day-light: but, if you intercept the Blue at the lens, the infusion will lose its reflected Blue colour, whilst its transmitted Red remains perfect, and by the loss of some Blue-making rays, wherewith it was allayed, becomes more intense and full. And, on the contrary, if the Red and Orange-making rays be intercepted at the lens, the infusion will lose its transmitted Red, whilst its Blue will remain, and become more full and perfect. Which shews, that the infusion does not tinge the rays with Blue and Red, but only transmits those most copiously which were Red-making before, and reflects those most copiously which were Blue-making before. And after the same manner may the reasons of other phenomena be examined, by trying them in this artificial beam of light, xy .

THE

THE
SECOND BOOK
OF
OPTICKS.

PART I.

Observations concerning the Reflexions, Refractions and Colours of thin transparent Bodies.*

IT has been observed by others, that Transparent Substances, as Glass, Water, Air, &c. when made very thin by being blown into bubbles, or otherwise formed into plates, do exhibit various colours according to their various thinness; although at a greater thickness they appear very clear and colourless. In the former Book I forbore to treat of these colours; because they seemed of a more difficult consideration, and were not necessary for establishing the properties of light there discoursed of. But because they may conduce to farther discoveries for compleating the theory of light, especially as to the constitution of the parts of Natural Bodies, on which their colours or transparency depend; I have here set down an account of them. To render this discourse short and distinct, I have first described the principal of my observations, and then considered and made use of them. The observations are these.

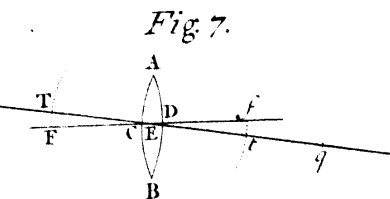
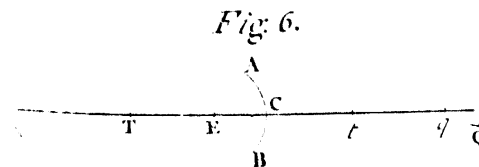
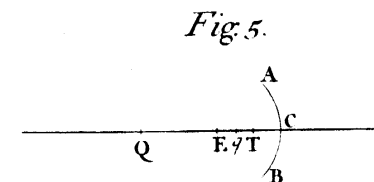
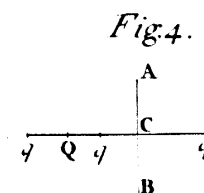
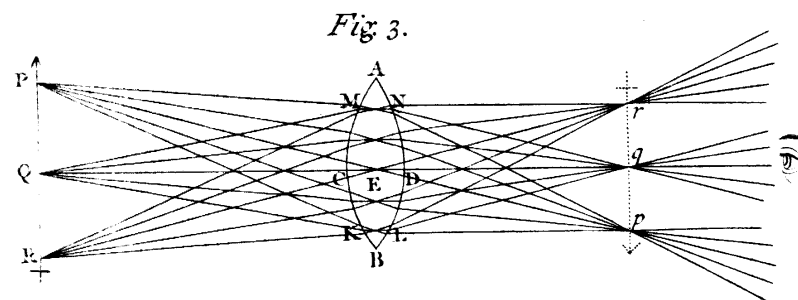
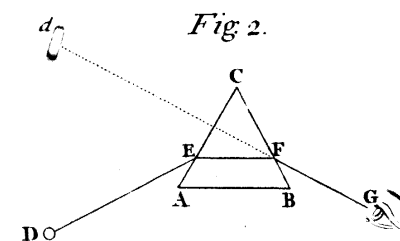
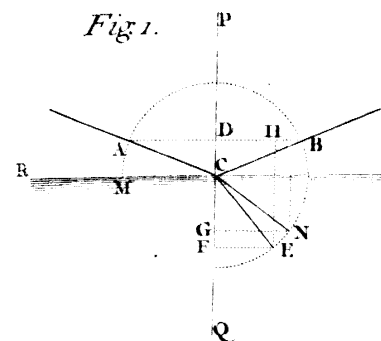


Fig. 8.

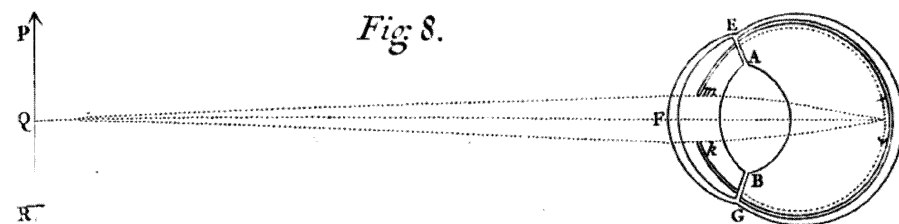


Fig. 9.

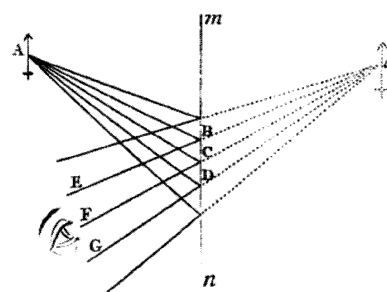


Fig. 11.

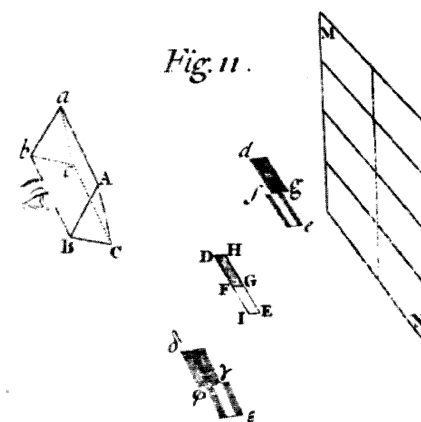


Fig. 10.

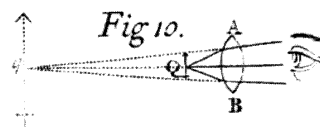
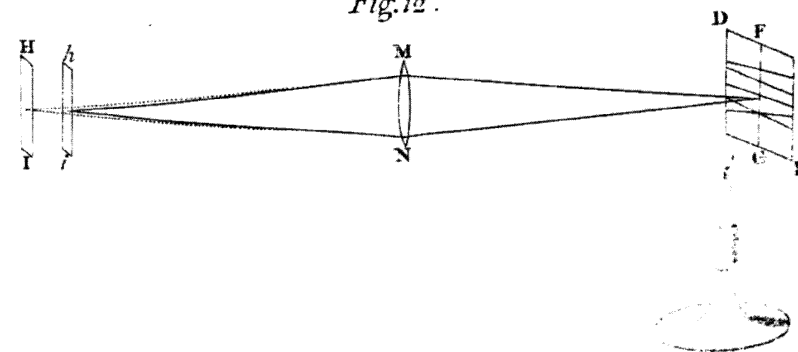
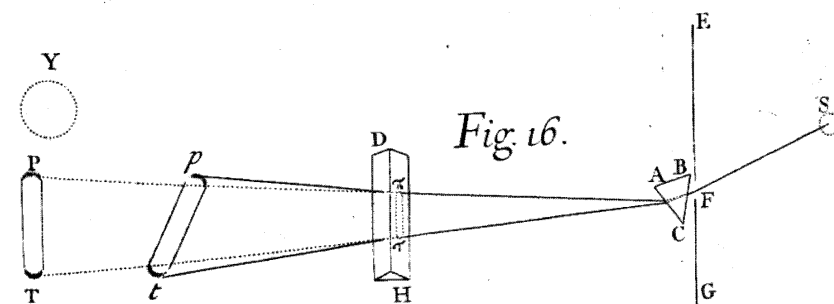
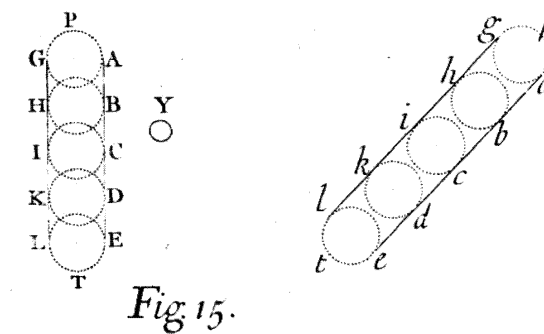
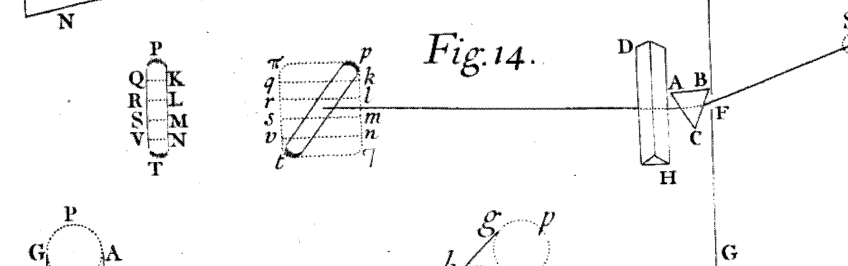
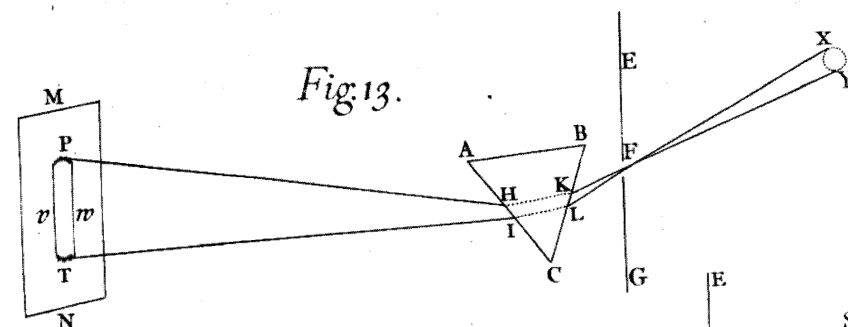


Fig. 12.





Obf. 1. Compressing two prisms hard together, that their sides, ^{Prisms} which by chance were a very little convex, might somewhere ^{compressed.} touch one another: I found the place in which they touched to become absolutely transparent, as if they had there been one continued piece of glass. For when the light fell so obliquely on the air, which in other places was between them, as to be all reflected, it seemed in that place of contact to be wholly transmitted; inasmuch that when looked upon, it appeared like a black or dark spot, by reason that little or no sensible light was reflected from thence, as from other places; and, when looked through, it seemed as it were a hole in that air, which was formed into a thin plate by being compressed between the glasses. And through this hole objects, that were beyond, might be seen distinctly, which could not at all be seen through other parts of the glasses where the air was interjacent. Although the glasses were a little convex, yet this transparent spot was of a considerable breadth, which breadth seemed principally to proceed from the yielding inwards of the parts of the glasses, by reason of their mutual pressure. For by pressing them very hard together it would become much broader than otherwise.

Obf. 2. When the plate of air, by turning the prisms about ^{Rings of} their common axis, became so little inclined to the incident rays, ^{Colours.} that some of them began to be transmitted; there arose in it many slender arcs of colours which at first were shaped almost like the conchoid, as you see them delineated in the first figure. And by continuing the motion of the prisms, these arcs increased, and bended more and more about the said transparent spot, till they were completed into circles or rings encompassing it, and afterwards continually grew more and more contracted.

These arcs at their first appearance were of a Violet and Blue ^{Order of the} colour, and between them were White arcs of circles, which presently, by continuing the motion of the prisms, became a little tinged in their inward limbs with Red and Yellow, and to their outward limbs the Blue was adjacent. So that the order of these colours, from the central dark spot, was at that time White, Blue, Violet; Black, Red, Orange, Yellow, White, Blue, Violet, &c.

&c. But the Yellow and Red were much fainter than the Blue and Violet.

The motion of the prisms about their axis being continued, these colours contracted more and more, shrinking towards the Whiteness on either side of it, until they totally vanished into it. And then the circles in those parts appeared Black and White, without any other colours intermixed. But by farther moving the prisms about, the colours again emerged out of the Whiteness, the Violet and Blue at its inward limb, and at its outward limb the Red and Yellow. So that now their order from the central spot was White, Yellow, Red, Black, Violet, Blue, White, Yellow, Red, &c. contrary to what it was before.

Number of
the Rings.

Obf. 3. When the rings or some parts of them appeared only Black and White, they were very distinct and well defined, and the Blackness seemed as intense as that of the central spot. Also in the borders of the rings, where the colours began to emerge out of the Whiteness, they were pretty distinct, which made them visible to a very great multitude. I have sometimes numbered above thirty successions, reckoning every Black and White ring for one succession, and seen more of them, which by reason of their smallness I could not number. But in other positions of the prisms, at which the rings appeared of many colours, I could not distinguish above eight or nine of them, and the exterior of those were very confused and dilute.

In these two Observations to see the rings distinct, and without any other colour than Black and White, I found it necessary to hold my eye at a good distance from them. For by approaching nearer, although in the same inclination of my eye to the plane of the rings, there emerged a Blueish colour out of the White; which, by dilating itself more and more into the Black, rendered the circles less distinct, and left the White a little tinged with Red and Yellow. I found also by looking through a slit or oblong hole, which was narrower than the pupil of my eye, and held close to it, parallel to the prisms; I could see the circles much distincter, and visible to a far greater number than otherwise.

Object-glasses
compressed.

Obf. 4. To observe more nicely the order of the circles, which arose out of the White circles, as the rays became less and less inclined

inclined to the plate of air; I took two object-glasses, the one a plano-convex for a fourteen foot telescope, and the other a large double convex for one of about fifty foot; and upon this, laying the other with its plane side downwards, I pressed them slowly together, to make the colours successively emerge in the middle of the circles, and then slowly lifted the upper glass from the lower to make them successively vanish again in the same place. The colour, which by pressing the glasses together emerged last in the middle of the other colours, would, upon its first appearance, look like a circle of a colour almost uniform from the circumference to the center; and, by compressing the glasses still more, grow continually broader until a new colour emerged in its center, and thereby it became a ring encompassing that new colour. And by compressing the glasses still more, the diameter of this ring would increase, and the breadth of its orbit or perimeter decrease, until another new colour emerged in the center of the last: and so on until a third, a fourth, a fifth, and other following new colours successively emerged there, and became rings encompassing the innermost colour, the least of which was the Black spot. And, on the contrary, by lifting up the upper glass from the lower, the diameter of the rings would decrease, and the breadth of their orbit increase, until their colours reached successively to the center; and then they being of a considerable breadth, I could more easily discern and distinguish their species than before. And by this means I observed their succession and quantity to be as followeth.

Next to the pellucid central spot, made by the contact of the glasses, succeeded Blue, White, Yellow and Red. The Blue was so little in quantity that I could not discern it in the circles made by the prisms, nor could I well distinguish any Violet in it; but the Yellow and Red were pretty copious, and seemed about as much in extent as the White, and four or five times more than the Blue. The next circuit in order of colours, immediately encompassing these, were Violet, Blue, Green, Yellow and Red: and these were all of them copious and vivid, excepting the Green, which was very little in quantity, and seemed much more fair and dilute than the other colours. Of the other four, the Violet was

Succession and
Quantity of
the Colours.

the least in extent, and the Blue less than the Yellow or Red. The third circuit or order was Purple, Blue, Green, Yellow and Red; in which the Purple seemed more reddish than the Violet in the former circuit, and the Green was much more conspicuous, being as brisk and copious as any of the other colours, except the Yellow; but the Red began to be a little faded, inclining very much to Purple. After this succeeded the fourth circuit of Green and Red. The Green was very copious and lively, inclining on the one side to Blue, and on the other side to Yellow. But in this fourth circuit there was neither Violet, Blue, nor Yellow, and the Red was very imperfect and dirty. Also the succeeding colours became more and more imperfect and dilute, till after three or four revolutions they ended in perfect Whiteness. Their form, when the glasses were most compressed so as to make the Black spot appear in the center, is delineated in the second figure; where $a, b, c, d, e: f, g, h, i, k: l, m, n, o, p: q, r: s, t: v, x: y, z$ denote the colours reckoned in order from the center; Black, Blue, White, Yellow, Red: Violet, Blue, Green, Yellow, Red: Purple, Blue, Green, Yellow, Red: Green, Red: greenish Blue, Red: greenish Blue, pale Red: greenish Blue, reddish White.

Thickness of
the Plates of
Air producing

Obs. 5. To determine the interval of the glasses, or thickness of the interjacent air, by which each colour was produced, I measured the diameters of the first six rings at the most lucid part of their orbits; and squaring them, I found their squares to be in the arithmetical progression of the odd numbers, 1, 3, 5, 7, 9, 11. And since one of these glasses was plane, and the other spherical, their intervals at those rings must be in the same progression. I measured also the diameters of the dark or faint rings between the more lucid colours, and found their squares to be in the arithmetical progression of the even numbers, 2, 4, 6, 8, 10, 12. And it being very nice and difficult to take these measures exactly; I repeated them divers times at divers parts of the glasses, that by their agreement I might be confirmed in them. And the same method I used in determining some others of the following Observations.

Obs.

Obs. 6. The diameter of the sixth ring, at the most lucid part of its orbit, was $\frac{58}{100}$ parts of an inch; and the diameter of the sphere on which the double convex object-glass was ground was about 102 feet; and hence I gathered the thickness of the air, or aerial interval of the glasses at that ring. But some time after, suspecting that in making this Observation I had not determined the diameter of the sphere with sufficient accurateness, and being uncertain whether the plano-convex glass was truly plane, and not something concave or convex on that side which I accounted plane; and whether I had not pressed the glasses together, as I often did, to make them touch; (for by pressing such glasses together their parts easily yield inwards, and the rings thereby become sensibly broader than they would be, did the glasses keep their figures) I repeated the experiment; and found the diameter of the sixth lucid ring about $\frac{55}{100}$ parts of an inch. I repeated the experiment also with such an object-glass of another telescope as I had at hand. This was a double convex, ground on both sides to one and the same sphere, and its focus was distant from it $83\frac{2}{3}$ inches. And thence, if the sines of incidence and refraction of the bright Yellow light be assumed in proportion as 11 to 17, the diameter of the sphere, to which the glass was figured, will by computation be found 182 inches. This glass I laid upon a flat one, so that the Black spot appeared in the middle of the rings of colours, without any other pressure than that of the weight of the glass. And now measuring the diameter of the fifth dark circle, as accurately as I could, I found it the fifth part of an inch precisely. This measure was taken with the points of a pair of compasses on the upper surface on the upper glass; and my eye was about eight or nine inches distant from the glass, almost perpendicularly over it; and the glass was $\frac{1}{6}$ of an inch thick: and thence it is easy to collect that the true diameter of the ring between the glasses was greater than its measured diameter above the glasses, in the proportion of 80 to 79, or thereabouts; and by consequence equal to $\frac{6}{79}$ part of an inch, and its true semidiameter equal to $\frac{8}{79}$ parts. Now as the diameter of the sphere, 182 inches, is to the semi-diameter of this fifth dark ring, $\frac{8}{79}$ parts of an inch, so is this semi-diameter to the thickness

Thickness of
the Plates of
Air

nefs of the air at this fifth dark ring; which is therefore $\frac{32}{567934}$ or $\frac{100}{1774784}$ parts of an inch; and the fifth part thereof, viz. the $\frac{1}{88739}$ part of an inch, is the thickness of the air at the first of these dark rings.

The same experiment I repeated with another double convex object-glass, ground on both sides to one and the same sphere. Its focus was distant from it $168\frac{1}{2}$ inches, and therefore the diameter of that sphere was 184 inches. The glass being laid upon the same plain glass, the diameter of the fifth of the dark rings, when the black spot in their center appeared plainly without pressing the glasses, was, by the measure of the compasses upon the upper glass, $\frac{121}{6000}$ parts of an inch; and by consequence between the glasses it was $\frac{1222}{6000}$. For the upper glass was $\frac{1}{8}$ of an inch thick, and my eye was distant from it 8 inches. And a third proportional to half this from the diameter of the sphere is $\frac{5}{88850}$ parts of an inch. This is therefore the thickness of the air at this ring, and a fifth part thereof, viz. the $\frac{1}{88850}$ th part of an inch is the thickness thereof at the first of the rings, as above.

I tried the same thing by laying these object-glasses upon flat pieces of a broken looking-glass, and found the same measures of the rings. Which makes me rely upon them, till they can be determined more accurately by glasses ground to larger spheres; though in such glasses greater care must be taken of a true plane.

These dimensions were taken when my eye was placed almost perpendicularly over the glasses, being about an inch, or an inch and a quarter, distant from the incident rays, and eight inches distant from the glass; so that the rays were inclined to the glass in an angle of about four degrees. Whence by the following Observation you will understand, that had the rays been perpendicular to the glasses, the thickness of the air at these rings would have been less, in the proportion of the radius to the secant of four degrees, that is of 10000 to 10024. Let the thicknesses found be therefore diminished in this proportion, and they will become $\frac{1}{88952}$ and $\frac{1}{89063}$; or, to use the nearest round number, the $\frac{1}{89000}$ th

(ⁿ) $84\frac{1}{3}$. I give this number as I find it in the third edition; which was the last published in our Author's life-time. The fourth edition, published in 1730 after his death, as was said from a copy

$\frac{1}{89000}$ th part of an inch. This is the thickness of the air at the darkest part of the First dark ring made by perpendicular rays: and half this thickness multiplied by the progression 1, 3, 5, 7, 9, 11, &c. gives the thicknesses of the air at the most luminous parts of all the brightest rings, viz. $\frac{1}{178000}$, $\frac{3}{178000}$, $\frac{5}{178000}$, $\frac{7}{178000}$, &c. their arithmetical means $\frac{4}{178000}$, $\frac{6}{178000}$, &c. being its thicknesses at the darkest parts of all the dark ones.

Obf. 7. The rings were least, when my eye was placed perpendicularly over the glasses in the axis of the rings: and when I

Angle of Incidence on the Air.	Angle of Refraction into the Air.	Diameter of the Ring.	Thickness of the Air.
Degr. Min.			
00 00	00 00	10	10
06 26	10 00	$10\frac{2}{3}$	$10\frac{2}{3}$
12 45	20 00	$10\frac{1}{2}$	$10\frac{1}{2}$
18 49	30 00	$10\frac{1}{3}$	$11\frac{1}{3}$
24 30	40 00	$11\frac{2}{3}$	13
29 37	50 00	$12\frac{1}{2}$	$15\frac{1}{2}$
33 58	60 00	14	20
35 47	65 00	$15\frac{1}{3}$	$23\frac{1}{3}$
37 19	70 00	$16\frac{2}{3}$	$28\frac{2}{3}$
38 33	75 00	$19\frac{1}{3}$	37
39 27	80 00	$22\frac{2}{3}$	$52\frac{2}{3}$
40 00	85 00	29	$84\frac{1}{3}$ (ⁿ)
40 11	90 00	35	$122\frac{1}{2}$

viewed them obliquely, they became bigger; continually swelling as I removed my eye farther from the axis. And partly by measuring the diameter of the same circle at several obliquities of my eye, partly by other means, as also by making use of the two prisms for very great obliquities, I found its diameter, and consequently the thickness of the air at its perimeter, in all

those obliquities, to be very nearly in the proportions expressed in this Table.

In the two first columns are expressed the obliquities of the incident and emergent rays to the plate of the air; that is, their angles of incidence and refraction. In the third column, the diameter of any coloured ring at those obliquities is expressed in parts, of which ten constitute that diameter, when the rays are perpendicular. And in the fourth column, the Thickness of the air, at the circumference of that ring, is expressed in parts of which also ten constitute its thickness, when the rays are perpendicular.

And from these measures I seem to gather this rule: That the Thickness of the air is proportional to the secant of an angle,

a copy of the third edition corrected by the Author's own hand, and left before his death with the Bookfeller, has $84\frac{1}{3}$. But the better reading I take to be $84\frac{1}{3}$, which was the reading both of the first and second edition, and of both the Latin editions of Dr. Clark.

whose

Thickness of
the Plates of
Air.

whose sine is a certain mean proportional between the sines of incidence and refraction. And that mean proportional, so far as by these measures I can determine it, is the first of an hundred and six arithmetical mean proportionals between those sines, counted from the bigger sine; that is, from the sine of refraction, when the refraction is made out of the glass into the plate of air; or from the sine of incidence, when the refraction is made out of the plate of air into the glass.

Obf. 8. The dark spot in the middle of the rings increased also by the obliquation of the eye, although almost insensibly. But if instead of the object-glasses the prisms were made use of, its increase was more manifest, when viewed so obliquely that no colours appeared about it. It was least, when the rays were incident most obliquely on the interjacent air, and as the obliquity decreased, it increased more and more until the coloured rings appeared; and then decreased again, but not so much as it increased before. And hence it is evident, that the transparency was not only at the absolute contact of the glasses, but also where they had some little interval. I have sometimes observed the diameter of that spot to be between half and two-fifth parts of the diameter of the exterior circumference of the Red in the first circuit or revolution of colours, when viewed almost perpendicularly; whereas, when viewed obliquely, it hath wholly vanished, and become opake and White like the other parts of the glass: whence it may be collected that the glasses did then scarcely, or not at all, touch one another; and that their interval at the perimeter of that spot, when viewed perpendicularly, was about a fifth or sixth part of their interval at the circumference of the said Red.

Ring of Co-
lours made

Obf. 9. By looking through the two contiguous object-glasses, I found that the interjacent air exhibited rings of colours, as well by transmitting light as by reflecting it. The central spot was now White, and from it the Order of the colours were yellowish Red; Black, Violet, Blue, White, Yellow, Red; Violet, Blue, Green, Yellow, Red, &c. But these colours were very faint and dilute, unless when the light was trajected very obliquely through the glasses: for by that means they became pretty vivid. Only the

the first yellowish Red, like the Blue in the fourth Observation, <sup>by transmi-
ted Light.</sup> was so little and faint as scarcely to be discerned. Comparing the coloured rings made by reflexion, with these made by Transmision of the light; I found that White was opposite to Black, Red to Blue, Yellow to Violet, and Green to a compound of Red and Violet. That is, those parts of the glass were black when looked through, which when looked upon appeared White, and on the contrary. And so those which in one case exhibited Blue, did in the other case exhibit Red. And the like of the other colours. The manner you have represented in the third figure, where AB, CD are the surfaces of the glasses contiguous at E, and the black lines between them are their distances in arithmetical progression, and the colours written above are seen by Reflected light, and those below by light Transmitted.

Obf. 10. Wetting the object-glasses a little at their edges, the water crept in slowly between them; and the circles thereby be- <sup>Rings of Co-
lours made by
Plates of
Water.</sup> came less, and the colours more faint: insomuch that as the water crept along, one half of them at which it first arrived, would appear broken off from the other half, and contracted into a less room. By measuring them, I found the proportions of their diameters to the diameters of the like circles made by air to be about seven to eight; and consequently the intervals of the glasses at like circles, caused by those two mediums water and air, are as about three to four. Perhaps it may be a general rule, that if any other medium more or less dense than water be compressed between the glasses, their intervals, at the rings caused thereby, will be to their intervals caused by interjacent air, as the sines are, which measure the refraction made out of that medium into air.

Obf. 11. When the water was between the glasses, if I pressed the upper glass variously at its edges, to make the rings move nimbly from one place to another, a little white spot would immediately follow the center of them, which upon creeping in of the ambient water into that place would presently vanish. Its appearance was such as interjacent air would have caused, and it exhibited the same colours. But it was not air, for where any bubbles of air were in the water they would not vanish. The reflexion must have rather been caused by a subtler medium, <sup>Indication of
a Medium
more subtil
than Air.</sup> which

which could recede through the glasses at the creeping in of the water.

The Rings
viewed

Obs. 12. These observations were made in the open air. But farther to examine the effects of coloured light falling on the glasses, I darkened the room, and viewed them by reflexion of the colours of a prism cast on a sheet of white paper; my eye being so placed that I could see the coloured paper by reflexion in the glasses, as in a looking-glass. And by this means the rings became distinct and visible to a far greater number than in the open air. I have sometimes seen more than twenty of them, whereas in the open air I could not discern above eight or nine.

Obs. 13. Appointing an assistant to move the prism to and fro about its axis, that all the colours might successively fall on that part of the paper, which I saw by reflexion, from that part of the glasses, where the circles appeared, so that all the colours might be successively reflected from the circles to my eye, whilst I held it immovable; I found the circles which the Red light made to be manifestly bigger than those which were made by the Blue and Violet. And it was very pleasant to see them gradually swell or contract accordingly as the colour of the light was changed. The interval of the glasses at any of the rings, when they were made by the outmost Red light, was to their interval at the same ring when made by the outmost Violet, greater than as 3 to 2, and less than as 13 to 8. By the most of my observations it was as 14 to 9. And this proportion seemed very nearly the same in all obliquities of my eye; unless when two prisms were made use of instead of the object-glasses. For then, at a certain great obliquity of my eye, the rings made by the several colours seemed equal; and at a greater obliquity, those made by the Violet would be greater than the same rings made by the Red: the refraction of the prism in this case causing the most refrangible rays to fall more obliquely on that plate of the air than the least refrangible ones. Thus the experiment succeeded in the coloured light, which was sufficiently strong and copious to make the rings sensible. And thence it may be gathered, that if the most refrangible and least refrangible rays had been copious enough to make the rings sensible without the mixture of other rays, the proportion

proportion which here was 14 to 9 would have been a little greater, suppose $14\frac{1}{4}$ or $14\frac{1}{3}$ to 9. by Prismatic Light.

Obs. 14. Whilst the prism was turned about its axis with an uniform motion, to make all the several colours fall successively upon the object-glasses, and thereby to make the rings contract and dilate: the contraction or dilatation of each ring, thus made by the variation of its colour, was swiftest in the Red, and slowest in the Violet, and in the intermediate colours it had intermediate degrees of celerity. Comparing the quantity of contraction and dilatation made by all the degrees of each colour, I found that it was greatest in the Red; less in the Yellow; still less in the Blue; and least in the Violet. And to make as just an estimation as I could of the proportions of their contractions or dilatations, I observed that the whole contraction or dilatation of the diameter of any ring, made by all the degrees of Red, was to that of the diameter of the same ring, made by all the degrees of Violet, as about four to three, or five to four; and that when the light was of the middle colour between Yellow and Green, the diameter of the ring was very nearly an arithmetical mean between the greatest diameter of the same ring made by the outmost Red, and the least diameter thereof made by the outmost Violet; contrary to what happens in the colours of the oblong spectrum made by the refraction of a prism, where the Red is most contracted, the Violet most expanded, and in the midst of all the colours is the confine of Green and Blue. And hence I seem to collect that the Thicknesses of the air between the glasses there, where the ring is successively made by the limits of the five principal colours, Red, Yellow, Green, Blue, Violet, in order; that is, by the extreme Red, by the limit of Red and Yellow in the middle of the Orange, by the limit of Yellow and Green, by the limit of Green and Blue, by the limit of Blue and Violet in the middle of the Indigo, and by the extreme Violet; are to one another very nearly as the six lengths of a chord which sound the notes in a sixth major, *sol, la, mi, fa, sol, la*. But it agrees something better with the observation to say, that the Thicknesses of the air between the glasses there, where the rings are successively made by the limits of the seven colours, Red, Orange, Yellow,

Yellow, Green, Blue, Indigo, Violet in order, are to one another as the cube-roots of the squares of the eight lengths of a chord, which found the notes in an eighth, *sol, la, fa, sol, la, mi, fa, sol*; that is, as the cube-roots of the squares of the numbers, $1, \frac{8}{9}, \frac{5}{6}, \frac{3}{4}, \frac{2}{3}, \frac{3}{5}, \frac{9}{16}, \frac{1}{2}$.

Obf. 15. These rings were not of various colours, like those made in the open air, but appeared all over of that Prismatic colour only with which they were illuminated. And by projecting the Prismatic colours immediately upon the glasses, I found that the light which fell on the dark spaces, which were between the coloured rings, was transmitted through the glasses without any variation of colour. For on a white paper placed behind, it would paint rings of the same colour with those which were reflected, and of the bigness of their immediate (°) spaces. And from thence the origin of these rings is manifest; namely, that the air between the glasses, according to its various Thickness, is disposed in some places to reflect, and in others to transmit the light of any one colour, as you may see represented in the fourth figure; and in the same place to reflect that of one colour, where it transmits that of another.

Obf. 16. The squares of the diameters of these rings, made by any prismatic colour, were in arithmetical progression, as in the fifth Observation. And the diameter of the sixth circle, when made by the Citrine Yellow, and viewed almost perpendicularly, was about $\frac{4.8}{100}$ parts of an inch, or a little less, agreeable to the sixth Observation.

The precedent Observations were made with a rarer thin medium, terminated by a denser, such as was air or water compressed between two glasses. In those that follow are set down the appearances of a denser medium thinned within a rarer; such as are plates of Muscovy glass, bubbles of water, and some other thin substances terminated on all sides with air.

Obf. 17. If a bubble be blown with water first made tenacious by dissolving a little soap in it, it is a common observation, that after a while it will appear tinged with a great variety of colours. To defend these bubbles from being agitated by the external air

(°) Perhaps intermediate.

(whereby their colours are irregularly moved one among another, so that no accurate observation can be made of them) as soon as I had blown any of them I covered it with a clear glass, and by that means its colours emerged in a very regular order, like so many concentrick rings encompassing the top of the bubble. And as the bubble grew thinner, by the continual subsiding of the water, these rings dilated slowly and overspread the whole bubble, descending in order to the bottom of it, where they vanished successively. In the mean while, after all the colours were emerged at the top, there grew in the center of the rings a small round Black spot, like that in the first Observation; which continually dilated itself till it became sometimes more than $\frac{1}{2}$ or $\frac{3}{4}$ of an inch in breadth, before the bubble broke. At first I thought there had been no light reflected from the water in that place; but observing it more curiously, I saw within it several smaller round spots, which appeared much blacker and darker than the rest; whereby I knew that there was some reflexion at the other places which were not so dark as those spots. And by farther trial I found that I could see the images of some things (as of a candle or the sun) very faintly reflected, not only from the great Black spot, but also from the little darker spots which were within in it.

Besides the aforesaid coloured rings there would often appear small spots of colours, ascending and descending up and down the sides of the bubble, by reason of some inequalities in the subsiding of the water. And sometimes small Black spots, generated at the sides, would ascend up to the larger Black spot at the top of the bubble, and unite with it.

Obf. 18. Because the colours of these bubbles were more extended and lively than those of the air thinned between two glasses, and so more easy to be distinguished, I shall here give you a farther description of their order, as they were observed in viewing them by reflexion of the skies, when of a White colour, whilst a black substance was placed behind the bubble. And they were these; Red, Blue; Red, Blue; Red, Blue; Red, Green; Red, Yellow, Green, Blue, Purple; Red, Yellow, Green, Blue, Violet; Red, Yellow, White, Blue, Black.

The three first successions of Red and Blue were very dilute and dirty, especially the first, where the Red seemed in a manner to be White. Among these there was scarce any other colour sensible besides Red and Blue, only the Blues, and principally the second Blue, inclined a little to Green.

The fourth Red was also dilute and dirty, but not so much as the former three; after that succeeded little or no Yellow, but a copious Green, which at first inclined a little to Yellow, and then became a pretty brisk and good Willow Green, and afterwards changed to a blueish colour; but there succeeded neither Blue nor Violet.

The fifth Red at first inclined very much to Purple, and afterwards became more bright and brisk, but yet not very pure. This was succeeded with a very bright and intense Yellow, which was but little in quantity, and soon changed to Green: but that Green was copious, and something more pure, deep and lively, than the former Green. After that followed an excellent Blue of a bright sky-colour, and then a Purple, which was less in quantity than the Blue, and much inclined to Red.

The sixth Red was at first of a very fair and lively Scarlet, and soon after of a brighter colour, being very pure and brisk, and the best of all the Reds. Then, after a lively Orange, followed an intense bright and copious Yellow; which was also the best of all the Yellows, and this changed first to a greenish Yellow, and then to a greenish Blue; but the Green between the Yellow and the Blue, was very little and dilute, seeming rather a greenish White than a Green. The Blue which succeeded became very good, and of a very fair bright sky-colour, but yet something inferior to the former Blue; and the Violet was intense and deep with little or no Redness in it. And less in quantity than the Blue.

In the last Red appeared a tincture of Scarlet next to Violet, which soon changed to a brighter colour, inclining to an Orange; and the Yellow which followed was at first pretty good and lively, but afterwards it grew more dilute, until by degrees it ended in perfect Whiteness. And this Whiteness, if the water was very tenacious and well tempered, would slowly spread and dilate itself over

over the greater part of the bubble, continually growing paler ^{in bubbles of Water.} at the top; where at length it would crack in many places; and those cracks, as they dilated, would appear of a pretty good, but yet obscure and dark sky-colour; the White between the Blue spots diminishing, until it resembled the threads of an irregular net-work, and soon after vanished and left all the upper part of the bubble of the said dark Blue colour. And this colour, after the aforesaid manner, dilated itself downwards, until sometimes it hath overspread the whole bubble. In the mean while at the top, which was of a darker Blue than the bottom, and appeared also full of many round blue spots, something darker than the rest, there would emerge one or more very Black spots; and within those, other spots of an intense Blackness, which I mentioned in the former Observation; and these continually dilated themselves until the bubble broke.

If the water was not very tenacious, the Black spots would break forth in the White, without any sensible intervention of the Blue. And sometimes they would break forth within the precedent Yellow, or Red, or perhaps within the Blue of the second order, before the intermediate colours had time to display themselves.

By this description you may perceive, how great an affinity these colours have with those of air described in the fourth Observation, although set down in a contrary order; by reason that they begin to appear when the bubble is thickest, and are most conveniently reckoned from the lowest and thickest part of the bubble upwards.

Obs. 19. Viewing, in several oblique positions of my eye, the rings of colours emerging on the top of the bubble, I found that they were sensibly dilated by increasing the Obliquity, but yet not so much by far as those made by thinned air in the seventh Observation. For there they were dilated so much as, when viewed most obliquely, to arrive at a part of the plate more than twelve times thicker, than that where they appeared when viewed perpendicularly; whereas in this case, the thickness of the water, at which they arrived when viewed most obliquely, was to that thickness which exhibited them by perpendicular rays, something less than as 8 to 5. By the best of my observations it

it was between 15 and $15\frac{1}{2}$ to 10 ; an increase about 24 times less than in the other case.

Sometimes the bubble would become of an uniform thickness all over, except at the top of it near the Black spot; as I knew, because it would exhibit the same appearance of colours in all positions of the eye. And then the colours, which were seen at its apparent circumference by the oblique rays, would be different from those that were seen in other places, by rays less oblique to it. And divers spectators might see the same part of it of differing colours, by viewing it at very differing obliquities. Now observing how much the colours at the same places of the bubble, or at divers places of equal thickness, were varied by

Incidence on the Water.		Refraction into the Water.		Thickness of the Water.
Degr.	Min.	Degr.	Min.	
00	00	00	00	10
15	00	11	11	$10\frac{1}{4}$
30	00	22	1	$10\frac{3}{8}$
45	00	32	2	$11\frac{1}{2}$
60	00	40	30	13
75	00	46	25	$14\frac{1}{2}$
90	00	48	35	$15\frac{1}{2}$

the several obliquities of the rays; by the assistance of the 4th, 14th, 16th and 18th Observations, as they are hereafter explained, I collect the Thickness of the water requisite to exhibit any one and the same colour, at several obliquities, to be very nearly in the proportion expressed in this Table.

In the two first columns are expressed the obliquities of the rays to the superficies of the water, that is, their angles of incidence and refraction. Where I suppose that the sines, which measure them, are in round numbers as 3 to 4; though probably the dissolution of soap in the water, may a little alter its Refractive virtue. In the third column the Thickness of the bubble, at which any one colour is exhibited in those several obliquities, is expressed in parts, of which ten constitute its Thickness when the rays are perpendicular. And the rule found by the seventh Observation agrees well with these measures, if duly applied; namely, that the Thickness of a plate of water requisite to exhibit one and the same colour at several obliquities of the eye, is proportional to the secant of an angle, whose sine is the first of an hundred and six arithmetical mean proportionals between the sines of incidence and refraction counted from the lesser sine,

fine, that is, from the sine of refraction, when the refraction is in bubbles of made out of air into water, otherwise from the sine of incidence. in bubbles of Water.

I have sometimes observed, that the colours which arise on polished steel by heating it, or on bell-metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air, have, like the colours of water-bubbles, been a little changed by viewing them at divers obliquities: and particularly that a deep Blue, or Violet, when viewed very obliquely, hath been changed to a deep Red. But the changes of these colours are not so great and sensible, as of those made by water. For the *Scoria*, or vitrified part of the metal, which most metals when heated or melted do continually protrude, and send out to their surface, and which, by covering the metals in form of a thin glassy skin, causes these colours, is much denser than water; and I find that the change, made by the obliquation of the eyes, is least in colours of the densest thin substances. Change of Colour by Obliquation of the Eye less in the denser substances.

Obf. 20. As in the ninth Observation so here, the bubble, by transmitted light, appeared of a contrary colour to that which it exhibited by reflexion. Thus when the bubble, being looked on by the light of the clouds reflected from it, seemed Red at its apparent circumference; if the clouds at the same time, or immediately after, were viewed through it, the colour at its circumference would be Blue. And, on the contrary, when by reflected light it appeared Blue, it would appear Red by transmitted light. Rings of Colours in bubbles of Water by transmitted Light.

Obf. 21. By wetting very thin plates of Muscovy glass, whose thinness make the like colours appear, the colours became more faint and languid; especially by wetting the plates on that side opposite to the eye; but I could not perceive any variation of their species. So then the Thickness of a plate requisite to produce any colour, depends only on the density of the plate, and not on that of the ambient medium. And hence, by the 10th and 16th Observations, may be known the Thickness which bubbles of water, or plates of Muscovy glass, or other substances, have at any colour produced by them. Species of Colour depends on the density of the Plate not on that of the ambient Medium.

Obf. 22. A thin transparent body, which is denser than its ambient medium, exhibits more brisk and vivid colours than that
Vol. IV. T which

which is so much rarer; as I have particularly observed in the air and glass. For blowing glass very thin at a lamp-furnace, those plates encompassed with air did exhibit colours much more vivid than those of air made thin between two glasses.

Obf. 23. Comparing the Quantity of light reflected from the several rings, I found that it was most copious from the first or innermost, and in the exterior rings became gradually less and less. Also the Whiteness of the first ring was stronger than that reflected from those parts of the thin medium or plate, which were without the rings; as I could manifestly perceive, by viewing at a distance the rings made by the two object-glasses; or by comparing two bubbles of water blown at distant times, in the first of which the Whiteness appeared, which succeeded all the colours, and in the other, the Whiteness which preceded them all.

Rings viewed

Obf. 24. When the two object-glasses were laid upon one another, so as to make the rings of the colours appear, though with my naked eye I could not discern above eight or nine of those rings, yet by viewing them through a prism I have seen a far greater multitude; inasmuch that I could number more than forty, besides many others, that were so very small and close together, that I could not keep my eye steady on them severally so as to number them, but by their extent I have sometimes estimated them to be more than an hundred. And I believe the Experiment may be improved to the discovery of far greater numbers. For they seem to be really unlimited, though visible only so far as they can be separated by the refraction of the prism, as I shall hereafter explain.

But it was but one side of these rings, namely, that towards which the refraction was made, which by that refraction was rendered distinct; and the other side became more confused than when viewed by the naked eye: inasmuch that there I could not discern above one or two, and sometimes none of those rings, of which I could discern eight or nine with my naked eye. And their segments or arcs, which on the other side appeared so numerous, for the most part exceeded not the third part of a circle. If the refraction was very great, or the prism very distant from the object-

object-glasses, the middle part of those arcs became also confused, ^{by Prismatic Light.} so as to disappear and constitute an even Whiteness: whilst on either side, their ends, as also the whole arcs farthest from the center, became distincter than before, appearing in the form as you see them designed in the fifth figure.

The arcs, where they seemed distinctest, were only White and Black successively, without any other colours intermixed. But in other places there appeared colours, whose order was inverted by the refraction in such manner, that if I first held the prism very near the object-glasses, and then gradually removed it farther off towards my eye, the colours of the 2d, 3d, 4th, and following rings shrunk towards the White that emerged between them, until they wholly vanished into it at the middle of the arcs, and afterwards emerged again in a contrary order. But at the ends of the arcs they retained their order unchanged.

I have sometimes so laid one object-glass upon the other, that to the naked eye they have all over seemed uniformly White, without the least appearance of any of the coloured rings; and yet by viewing them through a prism, great multitudes of those rings have discovered themselves. And in like manner plates of Muscovy glass, and bubbles of glass blown at a lamp-furnace, which were not so thin as to exhibit any colours to the naked eye, have through the prism exhibited a great variety of them ranged irregularly up and down in the form of waves. And so bubbles of water, before they began to exhibit their colours to the naked eye of a by-stander, have appeared through a prism, girded about with many parallel and horizontal rings; to produce which effect, it was necessary to hold the prism parallel, or very nearly parallel to the horizon, and to dispose it so, that the rays might be refracted upwards.

T H E
S E C O N D B O O K
O F
O P T I C K S.

P A R T II.

Remarks upon the foregoing Observations.

Generation of the Rings **H**AVING given my observations of these colours, before I make use of them to unfold the causes of the colours of Natural Bodies, it is convenient that by the simplest of them, such as are the 2d, 3d, 4th, 9th, 12th, 18th, 20th, and 24th, I first explain the more compounded. And first to shew how the colours in the fourth and eighteenth Observations are produced, let there be taken in any right line from the point γ [in *fig. 6.*] the lengths γA , γB , γC , γD , γE , γF , γG , γH , in proportion to one another, as the cube-roots of the squares of the numbers, $\frac{1}{5}$, $\frac{2}{10}$, $\frac{3}{15}$, $\frac{4}{20}$, $\frac{5}{25}$, $\frac{6}{30}$, $\frac{7}{35}$, $\frac{8}{40}$, $\frac{9}{45}$, $\frac{10}{50}$, $\frac{11}{55}$, $\frac{12}{60}$, $\frac{13}{65}$, $\frac{14}{70}$, $\frac{15}{75}$, $\frac{16}{80}$, $\frac{17}{85}$, $\frac{18}{90}$, $\frac{19}{95}$, $\frac{20}{100}$, whereby the lengths of a Musical chord to sound all the notes in an eighth are represented; that is, in the proportion of the numbers 6300, 6814, 7114, 7631, 8255, 8855, 9343, 10000. And at the points A , B , C , D , E , F , G , H , let perpendiculars $A\alpha$, $B\beta$, &c. be erected, by whose intervals the extent of the several colours set underneath against them, is to be represented.

represented. Then divide the line Az in such proportion as the numbers 1, 2, 3, 5, 6, 7, 9, 10, 11, &c. set at the points of division denote. And through those divisions from γ draw lines 1I, 2K, 3L, 5M, 6N, 7O, &c.

Now if $A2$ be supposed to represent the thickness of any thin transparent body, at which the outmost Violet is most copiously reflected in the first ring, or series of colours; then, by the 13th Observation (^o), HK will represent its thickness, at which the outmost Red is most copiously reflected in the same series. Also by the 5th and 16th Observations, A6 and HN will denote the thicknesses at which those extreme colours are most copiously reflected in the second series; and A10 and HQ the thicknesses, at which they are most copiously reflected in the third series, and so on. And the thickness at which any of the intermediate colours are reflected most copiously, will, according to the 14th Observation, be defined by the distance of the line AH from the intermediate parts of the lines 2K, 6N, 10Q, &c. against which the names of those colours are written below.

But farther, to define the latitude of these colours in each ring or series, let A1 design the least thickness, and A3 the greatest thickness, at which the extreme Violet in the First series is reflected; and let HI, and HL, design the like limits for the extreme Red; and let the intermediate colours be limited by the intermediate parts of the lines 1I, and 3L, against which the names of those colours are written, and so on: but yet with this caution, that the reflexions be supposed strongest at the intermediate spaces, 2K, 6N, 10Q, &c. and from thence to decrease gradually towards these limits, 1I, 3L, 5M, 7O, &c. on either side; where you must not conceive them to be precisely limited, but to decay indefinitely. And whereas I have assigned the same latitude to every series; I did it, because although the colours in the first series seem to be a little broader than the rest, by reason of a stronger reflexion there, yet that inequality is so insensible as scarcely to be determined by observation.

Now according to this description, conceiving that the rays, originally of several colours, are by turns reflected at the spaces

(^o) — by the 13th Observation.] Rather by the 14th.

Generations of the Rings
 11L3, 5MO7, 9PRII, &c. and transmitted at the spaces AHI, 3LM5, 7OP9, &c. it is easy to know what colour must, in the open air, be exhibited at any Thickness of a transparent thin body. For if a ruler be applied parallel to AH, at that distance from it, by which the thickness of the body is represented, the alternate spaces, 11L3, 5MO7, &c. which it crosseth, will denote the reflected original colours, of which the colour, exhibited in the open air, is compounded. Thus if the constitution of the Green in the Third series of colours be desired, apply the ruler as you see at $\pi\phi\phi$; and by its passing through some of the Blue at π , and Yellow at σ , as well as through the Green at ρ , you may conclude that the Green exhibited at that thickness of the body is principally constituted of original Green, but not without a mixture of some Blue and Yellow.

By this means you may know how the colours, from the center of the rings outward, ought to succeed in order as they were described in the 4th and 18th Observations. For if you move the ruler gradually from AH through all distances, having passed over the first space, which denotes little or no reflexion to be made by thinnest substances, it will first arrive at I the Violet; and then very quickly at the Blue and Green, which together with that Violet compound Blue; and then at the Yellow and Red, by whose farther addition that Blue is converted into Whiteness, which whiteness continues during the transit of the edge of the ruler from I to 3; and after that, by the successive deficiency of its component colours, turns first to compound Yellow, and then to Red, and last of all the Red ceaseth at L. Then begin the colours of the second series, which succeed in order during the transit of the edge of the ruler from 5 to O, and are more lively than before, because more expanded and seivered. And for the same reason, instead of the former White, there intercedes between the Blue and Yellow a mixture of Orange, Yellow, Green, Blue and Indigo, all which together ought to exhibit a dilute and imperfect Green. So the colours of the third series all succeed in order; first, the Violet, which a little interferes with the Red of the second order, and is thereby inclined to a reddish Purple; then the Blue and Green, which are less mixed with other colours,

lours, and consequently more lively than before, especially the Green: then follows the Yellow, some of which towards the Green is distinct and good, but that part of it towards the succeeding Red, as also that Red is mixed with the Violet and Blue of the fourth series; whereby various degrees of Red very much inclining to Purple are compounded. The Violet and Blue, which should succeed this Red, being mixed with, and hidden in it, there succeeds a Green. And this at first is much inclined to Blue, but soon becomes a good Green, the only unmixed and lively colour in this Fourth series. For as it verges towards the Yellow, it begins to interfere with the colours of the Fifth series; by whose mixture the succeeding Yellow and Red are very much diluted and made dirty, especially the Yellow, which, being the weaker colour, is scarce able to shew itself. After this the several series interfere more and more, and their colours become more and more intermixed, till after three or four more revolutions (in which the Red and Blue predominate by turns) all sorts of colours are in all places pretty equally blended, and compound an even Whiteness.

And since by the 15th Observation the rays endued with one colour are transmitted, where those of another colour are reflected, the reason of the colours made by the transmitted light in the 9th and 20th Observations is from hence evident.

If not only the order and species of these colours, but also the precise Thickness of the plate, or thin body at which they are exhibited, be desired in parts of an inch, that may be also obtained by assistance of the 6th or 16th Observations. For according to those Observations the thickness of the thinned air, which between two glasses exhibited the most luminous parts of the first six rings, were $\frac{1}{1783000}$, $\frac{1}{1780000}$, $\frac{1}{1786000}$, $\frac{1}{1780000}$, $\frac{1}{1780000}$, $\frac{1}{1780000}$ parts of an inch. Suppose the light reflected most copiously at these thicknesses be the bright citrine Yellow, or confine of Yellow and Orange; and these thicknesses will be $F\lambda$, $F\mu$, $F\xi$, $F\eta$, $F\theta$. And this being known, it is easy to determine what Thickness of air is represented by $\phi\phi$, or by any other distance of the ruler from AH.

Thickness of
coloured
Plates.

But farther, since by the 10th Observation the thickness of air was to the thickness of water, which between the same glasses exhibited the same colour, as 4 to 3; and by the 21st Observation the colours of thin bodies are not varied by varying the ambient medium: the Thickness of a bubble of water, exhibiting any colour, will be $\frac{3}{4}$ of the thickness of air producing the same colour. And so according to the same 10th and 21st Observations the Thickness of a plate of glass, whose refraction of the mean refrangible ray is measured by the proportion of the fines 31 to 20, may be $\frac{20}{31}$ of the thickness of air producing the same colours; and the like of other mediums. I do not affirm, that this proportion of 20 to 31 holds in all the rays; for the fines of other sorts of rays have other proportions. But the differences of those proportions are so little, that I do not here consider them. On these grounds I have composed the following Table, wherein the Thickness of air, water, and glass, at which each colour is most intense and specific, is expressed in parts of an inch divided into ten hundred thousand equal parts.

The thickness of coloured Plates and Particles of

and particles
of Air, Wa-
ter, and Glass.

		Air. Water. Glass.		
Their Colours of the first Order,	Very Black	$\frac{1}{2}$	$\frac{3}{8}$	$\frac{10}{31}$
	Black	1	$\frac{3}{4}$	$\frac{30}{31}$
	Beginning of Black	2	$1\frac{1}{2}$	$1\frac{7}{8}$
	Blue	$2\frac{2}{3}$	$1\frac{4}{5}$	$1\frac{11}{16}$
	White	$5\frac{1}{4}$	$3\frac{1}{2}$	$3\frac{3}{4}$
	Yellow	$7\frac{1}{9}$	$5\frac{1}{3}$	$4\frac{1}{2}$
	Orange	8	6	$5\frac{1}{2}$
	Red	9	$6\frac{1}{2}$	$5\frac{3}{4}$
Of the second Order,	Violet	$11\frac{1}{6}$	$8\frac{1}{8}$	$7\frac{1}{5}$
	Indigo	$12\frac{1}{6}$	$9\frac{1}{8}$	$8\frac{1}{10}$
	Blue	14	$10\frac{1}{2}$	9
	Green	$15\frac{1}{3}$	$11\frac{1}{2}$	$9\frac{1}{2}$
	Yellow	$16\frac{2}{3}$	$12\frac{1}{2}$	$10\frac{2}{3}$
	Orange	$17\frac{2}{3}$	13	$11\frac{1}{3}$
	Bright Red	$18\frac{1}{3}$ (P)	$13\frac{1}{2}$	$11\frac{5}{6}$
	Scarlet	$19\frac{2}{3}$	$14\frac{1}{2}$	$12\frac{2}{3}$
Of the third Order,	Purple	21	$15\frac{1}{2}$	$13\frac{11}{16}$
	Indigo	$22\frac{1}{10}$	$16\frac{1}{2}$	$14\frac{1}{5}$
	Blue	$23\frac{2}{5}$	$17\frac{11}{16}$	$15\frac{1}{16}$
	Green	$25\frac{1}{5}$	$18\frac{1}{2}$	$16\frac{1}{4}$
	Yellow	$27\frac{1}{7}$	$20\frac{1}{2}$	$17\frac{1}{2}$
	Red	29	$21\frac{1}{2}$	$18\frac{1}{2}$
	Blueish Red	32	24	$20\frac{1}{2}$
Of the fourth Order,	Blueish Green	24	$25\frac{1}{2}$	22
	Green	$35\frac{2}{7}$	$26\frac{1}{6}$	$22\frac{1}{3}$
	Yellowish Green	36	27	$23\frac{2}{9}$
	Red	$40\frac{1}{7}$	$30\frac{1}{4}$	26
Of the fifth Order,	Greenish Blue	46	$34\frac{1}{2}$	$29\frac{2}{3}$
	Red	$52\frac{1}{2}$	$39\frac{2}{3}$	34
Of the sixth Order,	Greenish Blue	$58\frac{1}{3}$	44	38
	Red	65	$48\frac{1}{2}$	42
Of the seventh Order,	Greenish Blue	71	$53\frac{1}{2}$	$45\frac{1}{2}$
	Ruddy White	77	$57\frac{1}{2}$	$49\frac{1}{2}$

Now if this Table be compared with the 6th Scheme, you will there see the constitution of each colour, as to its ingredients, or the original colours of which it is compounded; and thence be enabled to judge of its intenseness or imperfection; which may suffice in explication of the 4th and 18th Observations, unless it be farther desired to delineate the manner how the colours ap-

(P) Bright red 18 $\frac{1}{3}$.] So the third edition. All the rest 18 $\frac{1}{3}$.

pear, when the two object-glasses are laid upon one another. To do which, let there be described a large arc of a circle, and a straight line which may touch that arc, and parallel to that tangent several occult lines, at such distances from it, as the numbers set against the several colours in the Table denote. For the arc, and its tangent, will represent the superficies of the glasses terminating the interjacent air; and the places where the occult lines cut the arc will show at what distances from the center, or point of contact, each colour is reflected.

There are also other uses of this Table: for by its assistance the thickness of the bubble, in the 19th Observation, was determined by the colours which it exhibited. And so the bigness of the parts of Natural Bodies may be conjectured by their colours, as shall be hereafter shewn. Also if two or more very thin plates be laid upon one another, so as to compose one plate equalling them all in thickness, the resulting colour may be hereby determined. For instance, Mr. *Hook* observed, as is mentioned in his *Micrographia*, that a faint yellow plate of Muscovy glass, laid upon a blue one, constituted a very deep Purple. The Yellow of the First order is a faint one, and the thickness of the plate exhibiting it, according to the Table, is $4\frac{1}{3}$; to which add 9, the thickness exhibiting Blue of the Second order, and the sum will be $13\frac{1}{3}$, which is the thickness exhibiting the Purple of the Third order.

Transmutation of the Colours

To explain, in the next place, the circumstances of the 2d and 3d Observations; that is, how the rings of the colours may, by turning the prisms about their common axis the contrary way to that expressed in those Observations, be converted into White and Black rings, and afterwards into rings of colours again, the colours of each ring lying now in an inverted order; it must be remembered, that those rings of colours are dilated by the obliquation of the rays to the air which intercedes the glasses; and that according to the Table in the 7th Observation, their dilatation, or increase of their diameter, is most manifest and speedy when they are obliquest. Now the rays of Yellow being more refracted by the first superficies of the said air than those of Red, are thereby made more oblique to the second superficies, at which they

they are reflected to produce the coloured rings; and consequently the Yellow circle in each ring will be more dilated than the Red; and the excess of its dilatation will be so much the greater, by how much the greater is the obliquity of the rays, until at last it become of equal extent with the Red of the same ring. And for the same reason the Green, Blue and Violet, will be also so much dilated by the still greater obliquity of their rays, as to become all very nearly of equal extent with the Red, that is, equally distant from the center of the rings. And then all the colours of the same ring must be coincident, and by their mixture exhibit a White ring. And these White rings must have Black and Dark rings between them, because they do not spread and interfere with one another as before. And for that reason also they must become distinct and visible to far greater numbers. But yet the Violet, being obliquest, will be something more dilated in proportion to its extent than the other colours, and so very apt to appear at the exterior verges of the White.

Afterwards, by a greater obliquity of the rays, the Violet and Blue become more sensibly dilated than the Red and Yellow; and so being farther removed from the center of the rings, the colours must emerge out of the White in an order contrary to that which they had before; the Violet and Blue at the exterior limbs of each ring, and the Red and Yellow at the interior. And the Violet, by reason of the greatest obliquity of its rays, being in proportion most of all expanded, will soonest appear at the exterior limb of each White ring, and become more conspicuous than the rest. And the several series of colours belonging to the several rings, will, by their unfolding and spreading, begin again to interfere, and thereby render the rings less distinct, and not visible to so great numbers.

If instead of the prisms the object-glasses be made use of, the rings, which they exhibit, become not white and distinct by the Obliquity of the eye; by reason that the rays in their passage through that air, which intercedes the glasses, are very nearly parallel to those lines in which they were first incident on the glasses; and consequently the rays endued with several colours are not inclined

clined one more than another to that air, as it happens in the prisms.

Of the confusion of the Black and White rings, and the Colour on their edges produced by approaching the Eye.

There is yet another circumstance of these experiments to be considered, and that is why the Black and White rings, which when viewed at a distance appear distinct, should not only become confused by viewing them near at hand, but also yield a Violet colour at both the edges of every White ring. And the reason is, that the rays, which enter the eye at several parts of the pupil, have several obliquities to the glasses; and those which are most oblique, if considered apart, would represent the rings bigger than those which are the least oblique. Whence the breadth of the perimeter of every White ring is expanded outwards by the obliquest rays, and inwards by the least oblique. And this expansion is so much the greater by how much the greater is the difference of the obliquity; that is, by how much the pupil is wider, or the eye nearer to the glasses. And the breadth of the Violet must be most expanded, because the rays apt to excite a sensation of that colour are most oblique to a second or farther superficies of the thinned air, at which they are reflected, and have also the greatest variation of obliquity; which makes that colour soonest emerge out of the edges of the White. And as the breadth of every ring is thus augmented, the dark intervals must be diminished, until the neighbouring rings become contiguous, and are blended; the exterior first, and then those nearer the center; so that they can no longer be distinguished apart, but seem to constitute an even and uniform Whiteness.

The effects of the Prism in the 24th Obs. explained.

Among all the observations there is none accompanied with so odd circumstances as the twenty-fourth. Of those the principal are, that in thin plates, which to the naked eye seem of an even and uniform transparent Whiteness, without any terminations of shadows, the refraction of a prism should make rings of colours appear, whereas it usually makes objects appear coloured only there, where they are terminated with shadows, or have parts unequally luminous; and that it should make those rings exceedingly distinct and White, although it usually renders objects confused and coloured. The cause of these things you will understand by considering, that all the rings of colours are really in the

the plate, when viewed with the naked eye; although, by reason of the great breadth of their circumferences, they so much interfere and are blended together, that they seem to constitute an uniform Whiteness. But when the rays pass through the prism to the eye, the orbits of the several colours in every ring are refracted, some more than others, according to their degrees of refrangibility: by which means the colours on one side of the ring, that is in the circumference on one side of its center, become more unfolded and dilated, and those on the other side more complicated and contracted. And where by a due refraction they are so much contracted, that the several rings become narrower than to interfere with one another, they must appear distinct, and also white, if the constituent colours be so much contracted as to be wholly coincident. But, on the other side, where the orbit of every ring is made broader by the farther unfolding of its colours, it must interfere more with other rings than before, and so become less distinct.

To explain this a little farther, suppose the concentric circles av and bx [in *fig. 7.*] represent the Red and Violet of any order, which, together with the intermediate colours, constitute any one of these rings. Now these being viewed through a prism, the Violet circle bx will, by a greater refraction, be farther translated from its place than the Red av , and so approach nearer to it on that side of the circles, towards which the refractions are made. For instance, if the Red be translated to av , the Violet may be translated to bx , so as to approach nearer to it at x than before; and if the Red be farther translated to av , the Violet may be so much farther translated, to bx , as to convene with it at x ; and if the Red be yet farther translated to av , the Violet may be still so much farther translated, to $\beta\xi$, as to pass beyond it at ξ , and convene with it at e and f . And this being understood not only of the Red and Violet, but of all the other intermediate colours, and also of every revolution of those colours, you will easily perceive how those of the same revolution or order, by their nearness at xv and $\gamma\xi$, and their coincidence at xv , e and f , ought to constitute pretty distinct arcs of circles, especially at xv , or at e and f : and that they will appear severally at xv , and at xv exhibit

Effects of the exhibit Whiteness by their coincidence; and again appear severally Prism in Obs. 24 explained. at $\gamma\zeta$, but yet in a contrary order to that which they had before, and still retain beyond e and f . But, on the other side, at ab , ab , or $\alpha\beta$, these colours must become much more confused, by being dilated and spread, so as to interfere with those of other orders. And the same confusion will happen at $\gamma\zeta$, between e and f , if the refraction be very great, or the prism very distant from the object-glasses: in which case no parts of the rings will be seen, save only two little arcs at e and f , whose distance from one another will be augmented by removing the prism still farther from the object-glasses: and these little arcs must be distinctest and whitest at their middle; and at their ends, where they begin to grow confused they must be coloured. And the colours at one end of every arc must be in a contrary order to those at the other end, by reason that they cross in the intermediate White; namely, their ends, which verge towards $\gamma\zeta$, will be Red and Yellow on that side next the center, and Blue and Violet on the other side. But their other ends, which verge from $\gamma\zeta$, will on the contrary be Blue and Violet on that side towards the center, and on the other side Red and Yellow.

Now as all these things follow from the properties of light by a mathematical way of reasoning, so the truth of them may be manifested by Experiments. For in a dark room, by viewing these rings through a prism, by reflexion of the several Prismatic Colours, which an assistant causes to move to and fro upon a wall or paper, from whence they are reflected, whilst the spectator's eye, the prism and the object-glasses (as in the 13th Observation) are placed steady: the position of the circles, made successively by the several colours, will be found such, in respect of one another, as I have described in the figures $abxv$, or $abxv$, or $\alpha\beta\gamma$. And by the same method the truth of the explications of other Observations may be examined.

Cause of the Colours seen in small Fragments

By what hath been said, the like phænomena of water, and thin plates of glass may be understood. But in small fragments of those plates, there is this farther observable; that where they be flat upon a table, and are turned about their centers whilst they are viewed through a prism, they will in some postures exhibit

exhibit waves of various colours, and some of them exhibit these of Glass-plates viewed through a Prism. waves in one or two positions only, but the most of them do in all positions exhibit them, and make them for the most part appear almost all over the plates. The reason is, that the superficies of such plates are not even, but have many cavities and swellings, which how shallow soever do a little vary the thickness of the plate. For at the several sides of those cavities, for the reasons newly described, there ought to be produced waves in several postures of the prism. Now though it be but some very small and narrower parts of the glass, by which these waves for the most part are caused, yet they may seem to extend themselves over the whole glass: because from the narrowest of those parts, there are colours of several orders, that is of several rings, confusedly reflected; which, by refraction of the prism, are unfolded, separated, and according to their degrees of refraction, dispersed to several places, so as to constitute so many several waves, as there were divers orders of colours promiscuously reflected from that part of the glass.

These are the principal phænomena of thin plates or bubbles, whose explications depend on the properties of light, which I have heretofore delivered. And these you see do necessarily follow from them, and agree with them, even to their very least circumstances; and not only so, but do very much tend to their proof. Thus by the 24th Observation it appears, that the rays of several colours, made as well by thin plates or bubbles, as by refractions of a prism, have several degrees of refrangibility; whereby those of each order, which at the reflexion from the plate or bubble are intermixed with those of other orders, are separated from them by refraction, and associated together so as to become visible by themselves like arcs of circles. For if the rays were all alike refrangible, it is impossible that the Whiteness, which to the naked sense appears uniform, should, by refraction, have its parts transposed and ranged into those Black and White arcs.

It appears also that the unequal refractions of difform rays proceed not from any contingent irregularities; such as are veins, an uneven polish, or fortuitous position of the pores of glass; The Colorific disposition of the rays in-une and im-mutable. unequal

The Colour Disposition unequal and casual motions in the air or æther; the spreading, breaking, or dividing the same ray into many diverging parts, or the like. For, admitting any such irregularities, it would be impossible for refractions to render those rings so very distinct, and well defined, as they do in the 24th Observation. It is necessary therefore that every ray have its proper and constant degree of refrangibility connate with it, according to which its refraction is ever justly and regularly performed, and that several rays have several of those degrees.

And what is said of their refrangibility may be also understood of their reflexibility, that is of their dispositions to be reflected some at a greater, and others at a less thickness, of thin plates or bubbles; namely, that those dispositions are also connate with the rays, and immutable; as may appear by the 13th, 14th and 15th Observations compared with the 4th and 18th.

By the precedent Observations it appears also, that Whiteness is a dissimilar mixture of all colours, and that light is a mixture of rays endued with all those colours. For considering the multitude of the rings of colours, in the 3d, 12th and 24th Observations, it is manifest, that although in the 4th and 18th Observations there appear no more than eight or nine of those rings, yet there are really a far greater number, which so much interfere and mingle with one another, as after those eight or nine revolutions to dilute one another wholly, and constitute an even and sensibly uniform Whiteness. And consequently that Whiteness must be allowed a mixture of all colours, and the light, which conveys it to the eye, must be a mixture of rays endued with all those colours.

But farther, by the 24th Observation, it appears that there is a constant relation between colours and refrangibility; the most refrangible rays being Violet, the least refrangible Red, and those of intermediate colours having proportionably intermediate degrees of refrangibility. And by the 13th, 14th and 15th Observations, compared with the 4th or 18th, there appears to be the same constant relation between colour and reflexibility, the Violet being in like circumstances reflected at least thicknesses of any thin plate or bubble, the Red at greatest thicknesses, and the intermediate

intermediate colours at intermediate thicknesses. Whence it follows, that the colorifick dispositions of rays are also connate with them and immutable; and by consequence that all the productions and appearances of colours in the world are derived not from any Physical change caused in light by refraction or reflexion, but only from the various mixtures or separations of rays, by virtue of their different refrangibility or reflexibility. And in this respect the science of colours becomes a speculation as truly mathematical, as any other part of Opticks. I mean so far as they depend on the nature of light, and are not produced or altered by the power of imagination, or by striking or pressing the eye.

the Rays of Light innate and immutable.

T H E
S E C O N D B O O K
O F
O P T I C K S.

P A R T III.

Of the Permanent Colours of Natural Bodies, and the analogy between them and the colours of thin transparent plates.

I AM now come to another part of this design; which is to consider how the phenomena of thin transparent plates stand related to those of all other Natural Bodies. Of these bodies I have already told you, that they appear of divers colours, accordingly as they are disposed to reflect most copiously the rays originally endued with those colours. But their constitutions, whereby they reflect some rays more copiously than others, remain to be discovered, and these I shall endeavour to manifest in the following Propositions.

P R O P. I.

Those superficies of transparent bodies Reflect the greatest quantity of light, which have the greatest Refracting Power; that is, which intercede mediums that differ most in their Refractive Densities. And in the confines of equally refracting mediums there is no reflexion.

Analogy between
The analogy between reflexion and refraction will appear by considering, that when light passeth obliquely out of one medium

um into another, which refracts from the perpendicular, the greater is the difference of their Refractive Density, the less obliquity of incidence is requisite to cause a total reflexion. For as the sines are which measure the refraction, so is the sine of incidence at which the total reflexion begins, to the radius of the circle; and consequently that angle of incidence is least, where there is the greatest difference of the sines. Thus in the passing of light out of water into air, where the refraction is measured by the ratio of the sines 3 to 4, the total reflexion begins when the angle of incidence is about 48 degrees 35 minutes. In passing out of glass into air, where the refraction is measured by the ratio of the sines 20 to 31, the total reflexion begins when the angle of incidence is 40 degrees 10 minutes; and so in passing out of crystal, or more strongly refracting mediums into air, there is still a less obliquity requisite to cause a total reflexion. Superficies therefore which Refract most do soonest Reflect all the light which is incident on them, and so must be allowed most strongly reflexive.

But the truth of this Proposition will farther appear by observing, that in the superficies interceding two transparent mediums (such as are air, water, oil, common glass, crystal, metalline glasses, island glasses, white transparent arsenick, diamonds, &c.) the reflexion is stronger or weaker accordingly, as the superficies hath a greater or less Refracting power. For in the confine of air and sal-gem it is stronger than in the confine of air and water; and still stronger in the confine of air and common glass or crystal; and stronger in the confine of air and a diamond. If any of these, and such like transparent solids, be immersed in water, its reflexion becomes much weaker than before; and still weaker, if they be immersed in the more strongly Refracting liquors of well rectified oil of vitriol or spirit of turpentine. If water be distinguished into two parts, by any imaginary surface, the reflexion in the confine of those two parts, is none at all. In the confine of water and ice it is very little; in that of water and oil it is something greater; in that of water and sal-gem still greater; and in that of water and glass, or crystal, or other denser substances still greater, accordingly as those mediums differ more or less in their Refracting powers. Hence in the confine of common

mon glafs and crystal, there ought to be a weak reflexion; and a stronger reflexion in the confine of common and metalline glafs, though I have not yet tried this. But, in the confine of two glaffes of equal density, there is not any fenfible reflexion, as was fhewn in the firft Obfervation. And the fame may be underftood of the fuperficies interceding two cryftals, or two liquors, or any other fubftances in which no refraction is caufed. So then the reafon why uniform pellucid mediums, fuch as water, glafs or crystal, have no fenfible reflexion but in their external fuperficies, where they are adjacent to other mediums of a different density, is becaufe all their contiguous parts have one and the fame degree of density.

P R O P. II.

The leaft parts of almoft all Natural Bodies are in fome meafure transparent: and the Opacity of thofe bodies arifeth from the multitude of reflexions caufed in their internal parts.

Cause of
Opacity

That this is fo has been obferved by others, and will eafily be granted by them that have been converfant with microfcopes. And it may be alfo tried by applying any fubftance to a hole, through which fome light is immitted into a dark room. For how Opaque foever that fubftance may feem in the open air, it will by that means appear very manifefly transparent, if it be of a fufficient thinnefs. Only White Metalline bodies muft be excepted, which, by reafon of their exceffive density, feem to reflect almoft all the light incident on their firft fuperficies, unlefs by folution in menftruums they be reduced into very fmall particles, and then they become transparent.

P R O P.

P R O P. III.

Between the parts of Opaque and Coloured bodies are many fpaces, either empty, or replenifhed with mediums of other densities; as water between the tinging corpuscles wherewith any liquor is impregnated; air between the aqueous globules that conftitute clouds or mifts; and for the moft part fpaces void of both air and water, but yet perhaps not wholly void of all fubftance, between the parts of Hard Bodies.

The truth of this is evinced by the two precedent Propofitions: Poroufnefs a principal cause of Opacity. for by the fecond Propofition, there are many reflexions made by the internal parts of bodies, which, by the firft Propofition, would not happen if the parts of thofe bodies were continued without any fuch interftices between them, becaufe reflexions are caufed only in fuperficies, which intercede mediums of a differing density by Prop. I.

But farther, that this difcontinuity of parts is the principal caufe of the Opacity of bodies, will appear by confidering, that Opaque fubftances become Transparent, by filling their pores with any fubftance of equal, or almoft equal, density with their parts. Thus paper dipped in water or oil, the *Oculus Mundi* ftone fteeped in water, linen cloth oiled or varnifhed, and many other fubftances soaked in fuch liquors as will intimately pervade their little pores, become by that means more transparent than otherwife; fo, on the contrary, the moft Transparent fubftances may, by evacuating their pores, or feparating their parts, be rendered fufficiently Opaque; as falts or wet paper, or the *Oculus Mundi* ftone, by being dried; horn, by being fcraped; glafs, by being reduced to powder, or otherwife flawed; turpentine, by being ftirred about with water till they mix imperfectly; and water, by being formed into many fmall bubbles, either alone in the form of froth, or by fhaking it together with oil of turpentine, or oil olive, or with fome other convenient liquor, with which it will not perfectly incorporate. And to the increafe of the Opacity of thefe bodies it conduces fomething, that, by the 23d Obfervation, the reflexions of

of very thin transparent substances are considerably stronger than those made by the same substances of a greater thickness.

P R O P. IV.

The Parts of bodies and their interstices must not be less than of some definite bigness, to render them Opaque and Coloured.

A definite size of Particles and Pores requisite to Opacity and Colour.

For the Opakest bodies, if their parts be subtilly divided (as metals by being dissolved in acid menstruums, &c.) become perfectly Transparent. And you may also remember, that in the eighth Observation there was no sensible reflexion at the superficies of the object-glasses, where they were very near one another, though they did not absolutely touch. And in the 17th Observation the reflexion of the water-bubble where it became thinnest was almost insensible, so as to cause very Black spots to appear on the top of the bubble by the want of reflected light.

On these grounds I perceive it is, that water, salt, glass, stones, and such like substances, are transparent. For, upon divers considerations, they seem to be as full of pores, or interstices between their parts, as other bodies are; but yet their parts and interstices to be too small to cause reflexions in their common surfaces.

P R O P. V.

The Transparent Parts of bodies according to their several sizes reflect rays of one colour, and transmit those of another, on the same grounds that thin plates or bubbles do reflect or transmit those rays. And this I take to be the ground of all their colours.

The transparent Parts of bodies reflect or transmit

For if a thinned or plated body, which being of an even thickness, appears all over of one uniform colour, should be slit into threads, or broken into fragments, of the same thickness with the plate; I see no reason why every thread or fragment should not keep its colour, and by consequence why a heap of those threads or fragments should not constitute a mass or powder of the same colour, which the plate exhibited before it was broken. And the parts of all Natural Bodies being like so many fragments

fragments of a plate, must on the same grounds exhibit the same colours.

the several Colorific rays according to their sizes.

Now that they do so, will appear by the affinity of their properties. The finely coloured feathers of some birds, and particularly those of peacocks tails, do in the very same part of the feather appear of several colours in several positions of the eye, after the very same manner that thin plates were found to do in the 7th and 19th Observations, and therefore their colours arise from the thinness of the transparent parts of the feathers; that is, from the slenderness of the very fine hairs, or *Capillamenta*, which grow out of the sides of the grosser lateral branches or fibres of those feathers. And to the same purpose it is, that the webs of some spiders, by being spun very fine, have appeared coloured, as some have observed, and that the coloured fibres of some silks, by varying the position of the eye, do vary their colour. Also the colours of silks, cloths, and other substances, which water or oil can intimately penetrate, become more faint and obscure by being immersed in those liquors, and recover their vigour again by being dried; much after the manner declared of thin bodies in the 10th and 21st Observations. Leaf-gold, some sorts of painted glass, the infusion of *Lignum Nepbriticum*, and some other substances reflect one colour, and transmit another, like thin bodies in the 9th and 20th Observations. And some of those coloured powders which painters use, may have their colours a little changed, by being very elaborately and finely ground. Where I see not what can be justly pretended for those changes, besides the breaking of their parts into less parts, by that contrition, after the same manner that the colour of a thin plate is changed, by varying its thickness. For which reason also it is that the coloured flowers of plants and vegetables by being bruised usually become more transparent than before; or at least in some degree or other change their colours. Nor is it much less to my purpose, that by mixing divers liquors very odd and remarkable productions and changes of colours may be effected; of which no cause can be more obvious and rational, than that the saline corpuscles of one liquor do variously act upon, or unite with, the tinging corpuscles of another, so as to make them swell or shrink; whereby

whereby not only their bulk but their density also may be changed; or to divide them into smaller corpuscles, whereby a coloured liquor may become transparent; or to make many of them associate into one cluster, whereby two transparent liquors may compose a coloured one. For we see how apt those saline menstruums are to penetrate and dissolve substances to which they are applied, and some of them to precipitate what others dissolve. In like manner, if we consider the various phenomena of the atmosphere, we may observe, that when vapours are first raised, they hinder not the transparency of the air, being divided into parts too small to cause any reflexion in their superficies. But when in order to compose drops of rain they begin to coalesce and constitute globules of all intermediate sizes, those globules, when they become of a convenient size to reflect some colours and transmit others, may constitute clouds of various colours according to their sizes. And I see not what can be rationally conceived in so transparent a substance as water for the production of these colours, besides the various sizes of its fluid and globular parcels.

P R O P. VI.

The Parts of bodies on which their colours depend, are denser than the medium, which pervades their interstices.

Colorific
parts of Bo-
dies denser
than the Me-
dium which
pervades their
Pores.

This will appear by considering, that the colour of a body depends not only on the rays which are incident perpendicularly on its parts, but on those also which are incident at all other angles. And that according to the 7th Observation, a very little variation of Obliquity will change the reflected colour, where the thin body, or small particle is, at diversely oblique incidences, reflect of all sorts of colours, in so great a variety that the colour resulting from them all, confusedly reflected from a heap of such particles, must rather be a White or Grey than any other colour, or at best it must be but a very imperfect and dirty colour. Whereas if the thin body, or small particle, be much denser than the ambient medium, the colours according to the 19th Observation are so little changed by the variation of Obliquity, that the

rays, which are reflected least obliquely, may predominate over the rest, so much as to cause a heap of such particles to appear very intensely of their colour.

It conduces something also to the confirmation of this Proposition, that, according to the 22d Observation, the colours exhibited by the denser thin body, within the rarer, are more brisk than those exhibited by the rarer within the denser.

P R O P. VII.

The Bigness of the component Parts of Natural bodies may be conjectured by their colours.

For since the Parts of these bodies by Prop. v. do most probably exhibit the same colours with a plate of equal thickness, provided they have the same Refractive Density; and since their Parts seem for the most part to have much the same density with water or glass, as by many circumstances is obvious to collect; to determine the sizes of those Parts, you need only have recourse to the precedent Tables, in which the thickness of water or glass exhibiting any colour is expressed. Thus if it be desired to know the diameter of a corpuscle, which being of equal density with glass, shall reflect Green of the Third order; the number $16\frac{1}{4}$ shews it to be $\frac{16\frac{1}{4}}{1000000}$ parts of an inch.

The Colours
of Bodies in-
dicate the
sizes of their
component
particles.

The greatest difficulty is here to know of what Order the colour of any body is. And for this end we must have recourse to the 4th and 18th Observations, from whence may be collected these particulars.

Scarlets, and other *Reds*, *Oranges* and *Yellows*, if they be pure and intense, are most probably of the Second order. Those of the First and Third order also may be pretty good, only the Yellow of the first order is faint, and the Orange and Red of the Third order have a great mixture of Violet and Blue.

There may be good *Greens* of the Fourth order, but the purest are of the Third. And of this order the Green of all vegetables seem to be, partly by reason of the intenseness of their colours, and partly because when they wither, some of them turn to a greenish Yellow, and others to a more perfect Yellow or Orange,

The Colours
of Bodies
indicate

or perhaps to Red, passing first through all the aforesaid intermediate colours. Which changes seem to be effected by the exhaling of the moisture; which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy part of that moisture. Now the Green without doubt is of the same order, with those colours into which it changeth; because the changes are gradual, and those colours, though usually not very full, yet are often too full and lively to be of the Fourth order.

Blues and *Purples* may be either of the Second or Third order, but the best are of the Third. Thus the colour of Violets seems to be of that order, because their syrup by Acid liquors turns Red, and by Urinous and Alcalizate turns Green. For since it is of the nature of Acids to dissolve or attenuate, and of alcalies to precipitate or incrassate; if the Purple colour of the syrup was of the Second order, an Acid liquor, by attenuating its tinging corpuscles, would change it to a Red of the First order, and an alkali by incrassating them would change it to a Green of the second order; which Red and Green, especially the Green, seem too imperfect to be the colours produced by these changes. But if the said Purple be supposed of the Third order, its change to Red of the Second, and Green of the Third, may without any inconvenience be allowed.

If there be found any body of a deeper and less reddish Purple than that of the Violets, its colour most probably is of the Second order. But yet there being no body, commonly known, whose colour is constantly more deep than theirs; I have made use of their name to denote the deepest and least reddish Purples, such as manifestly transcend their colour in purity.

The *Blue* of the First order, though very faint and little, may possibly be the colour of some substances; and particularly the Azure colour of the skies seems to be of this order. For all vapours, when they begin to condense and coalesce into small parcels, become first of that bigness, whereby such an Azure must be reflected, before they can constitute clouds of other colours. And so this being the first colour which vapours begin to reflect, it ought to be the colour of the finest and most transparent skies,

in

in which vapours are not arrived to that grossness requisite to reflect other colours, as we find it is by experience.

Whiteness, if most intense and luminous, is that of the First order, if less strong and luminous a mixture of the colours of several orders. Of this last kind is the whiteness of froth, paper, linen, and most white substances; of the former I reckon that of white metals to be. For whilst the densest of metals, gold, if foliated, is transparent, and all metals become transparent, if dissolved in menstrua or vitrified; the opacity of white metals ariseth not from their density alone. They being less dense than gold would be more transparent than it, did not some other cause concur with their density to make them Opaque. And this cause I take to be such a bigness of their particles, as fits them to reflect the White of the First order. For if they be of other thickneses, they may reflect other colours; as is manifest by the colours which appear upon hot steel in tempering it, and sometimes upon the surface of melted metals, in the skin or scoria which arises upon them in their cooling. And as the White of the First order is the strongest which can be made by plates of transparent substances, so it ought to be stronger in the denser substances of metals, than in the rarer of air, water, and glass. Nor do I see but that Metallic substances, of such a thickness as may fit them to reflect the White of the First order, may, by reason of their great density, according to the tenour of the first of these Propositions, reflect all the light incident upon them, and so be as opaque and splendid as it is possible for any body to be. Gold, or Copper mixed with less than half their weight of silver, or tin, or regulus of antimony, in fusion, or amalgamed with a very little mercury, become white; which shews both that the particles of white metals have much more superficies, and so are smaller, than those of gold and copper, and also that they are so opaque, as not to suffer the particles of gold or copper to shine through them. Now it is scarce to be doubted, but that the colours of gold and copper are of the Second and Third order; and therefore the particles of white metals cannot be much bigger, than is requisite to make them reflect the White of the First order. The volatility of Mercury argues, that they are not much

the sizes of
their compo-
nent particles.

bigger; nor may they be much less, lest they lose their Opacity; and become either transparent, as they do when attenuated by vitrification, or by solution in Menstruums; or Black, as they do when ground smaller, by rubbing silver, or tin, or lead, upon other substances to draw Black lines. The first and only colour which white metals take by grinding their particles smaller, is Black; and therefore their White ought to be that which borders upon the Black spot in the center of the rings of colours, that is, the White of the First order. But if you would hence gather the bigness of Metallic particles, you must allow for their density. For were Mercury transparent, its density is such that the sine of incidence upon it (by my computation) would be to the sine of its refraction, as 71 to 20, or 7 to 2. And therefore the thickness of its particles, that they may exhibit the same colours with those of bubbles of water, ought to be less than the thickness of the skin of those bubbles, in the proportion of 2 to 7. Whence it is possible that the particles of Mercury may be as little as the particles of some transparent and volatile fluids, and yet reflect the White of the First order.

Lastly, for the production of *Black*, the corpuscles must be less than any of those which exhibit colours. For at all greater sizes there is too much light reflected to constitute this colour. But if they be supposed a little less than is requisite to reflect the White and very faint Blue of the First order, they will, according to the 4th, 8th, 17th and 18th Observations, reflect so very little light as to appear intensely Black; and yet may perhaps variously refract it to and fro within themselves so long, until it happen to be stifled and lost; by which means they will appear Black, in all positions of the eye, without any transparency. And from hence may be understood why fire, and the more subtle dissolver putrefaction, by dividing the particles of substances, turn them to Black; why small quantities of Black substances impart their colour very freely and intensely to other substances, to which they are applied; the minute particles of these, by reason of their very great number, easily overspreading the gross particles of others; why glass ground very elaborately with sand on a copper plate, till it be well polished, makes the sand, together

ther with what is worn off from the glass and copper, become very Black: why Black substances do soonest of all others become hot in the sun's light and burn (which effect may proceed partly from the multitude of refractions in a little room, and partly from the easy commotion of so very small corpuscles); and why Blacks are usually a little inclined to a blueish colour: for that they are so may be seen, by illuminating white paper by light reflected from Black substances. For the paper will usually appear of a blueish White; and the reason is, that Black borders on the obscure Blue of the First order described in the 18th Observation, and therefore reflects more rays of that colour, than of any other.

In these descriptions I have been the more particular, because it is not impossible but that Microscopes may at length be improved to the discovery of the particles of bodies on which their colours depend, if they are not already in some measure arrived to that degree of perfection. For if those instruments are or can be so far improved, as with sufficient distinctness to represent objects five or six hundred times bigger, than at a foot distance they appear to our naked eyes; I should hope that we might be able to discover some of the greatest of those corpuscles. And by one that would magnify three or four thousand times, perhaps they might all be discovered, but those which produce Blackness. In the mean while I see nothing material in this discourse that may rationally be doubted of, excepting this position: That transparent corpuscles of the same thickness and density with a plate, do exhibit the same colour. And this I would have understood not without some latitude; as well because those corpuscles may be of irregular figures, and many rays must be obliquely incident on them, and so have a shorter way through them than the length of their diameters; as because the straitness of the medium, pent in on all sides within such corpuscles, may a little alter its motions, or other qualities, on which the reflexion depends. But yet I cannot much suspect the last; because I have observed of some small plates of Muscovy-glass, which were of an even thickness, that through a microscope they have appeared of the same colour at their edges and corners, where the included medium

the sizes of
their compo-
nent particles.

Utmost pro-
bable im-
provement of
Microscopes.

dium was terminated, which they appeared of in other places. However it will add much to our satisfaction, if those corpuscles can be discovered with microscopes; which, if we shall at length attain to, I fear it will be the utmost improvement of this sense. For it seems impossible to see the more secret and noble works of Nature within the corpuscles, by reason of their transparency.

P R O P. VIII.

The cause of reflexion is not the impinging of light on the solid or impervious Parts of bodies, as is commonly believed.

The impact
of light
against the
solid particles

This will appear by the following considerations. First, That in the passage of light out of glass into air there is a reflexion as strong, as in its passage out of air into glass, or rather a little stronger; and by many degrees stronger, than in its passage out of glass into water. And it seems not probable, that air should have more strongly reflecting parts than water or glass. But if that should possibly be supposed, yet it will avail nothing; for the reflexion is as strong or stronger when the air is drawn away from the glass, suppose in the air-pump invented by *Otto Gueric* and improved and made useful by *Mr. Boyle*, as when it is adjacent to it. Secondly, If light in its passage out of glass into air be incident more obliquely, than at an angle of 40 or 41 degrees, it is wholly reflected; if less obliquely, it is in great measure transmitted. Now it is not to be imagined, that light, at one degree of obliquity, should meet with pores enough in the air to transmit the greater part of it; and at another degree of Obliquity, should meet with nothing but parts to reflect it wholly; especially considering that in its passage out of air into glass, how oblique soever be its incidence, it finds pores enough in the glass to transmit a great part of it. If any man suppose that it is not reflected by the air, but by the outmost superficial parts of the glass, there is still the same difficulty: besides, that such a supposition is unintelligible, and will also appear to be false by applying water behind some part of the glass instead of air. For so in a convenient obliquity of the rays, suppose of 45 or 46 degrees, at which they are all reflected where the air is adjacent to

to the glass; they shall be in great measure transmitted, where the water is adjacent to it: which argues, that their reflexion or transmission depends on the constitution of the air and water behind the glass, and not on the striking of the rays upon the parts of the glass. Thirdly, If the colours made by a prism, placed at the entrance of a beam of light into a darkened room, be successively cast on a second prism placed at a greater distance from the former, in such manner that they are all alike incident upon it; the second prism may be so inclined to the incident rays, that those which are of a Blue colour shall be all reflected by it, and yet those of a Red colour pretty copiously transmitted. Now if the reflexion be caused by the parts of air or glass, I would ask, why, at the same obliquity of incidence, the Blue should wholly impinge on those parts so as to be all reflected, and yet the Red find pores enough to be in a great measure transmitted. Fourthly, Where two glasses touch one another, there is no sensible reflexion, as was declared in the first Observation; and yet I see no reason, why the rays should not impinge on the parts of glass as much when contiguous to other glass, as when contiguous to air. Fifthly, When the top of a water-bubble, in the 17th Observation, by the continual subsiding and exhaling of the water grew very thin, there was such a little and almost insensible quantity of light reflected from it, that it appeared intensely Black; whereas round about that Black spot, where the water was thicker, the reflexion was so strong as to make the water seem very White. Nor is it only at the least thickness of thin plates or bubbles, that there is no manifest reflexion, but at many other thicknesses continually greater and greater. For in the 15th Observation the rays of the same colour were by turns transmitted at one thickness, and reflected at another thickness, for an indeterminate number of successions. And yet in the superficies of the thinned body, where it is of one thickness, there are as many parts for the rays to impinge on, as where it is of any other thickness. Sixthly, If reflexion were caused by the parts of reflecting bodies, it would be impossible for thin plates or bubbles at one and the same place to reflect the rays of one colour, and transmit those of another; as they do, according to the 13th and 15th Observations.

The impact of Light against the solid particles variations. For it is not to be imagined, that at one place the rays, which for instance exhibit a Blue colour, should have the fortune to dash upon the Parts, and those which exhibit a Red to hit upon the Pores of the body; and then at another place, where the body is either a little thicker, or a little thinner, that on the contrary the Blue should hit upon its Pores, and the Red upon its Parts. Lastly, were the rays of light reflected by impinging on the solid parts of bodies, their reflexions from polished bodies could not be so regular as they are. For in polishing glass with sand, putty or tripoly, it is not to be imagined, that those substances can, by grating and fretting the glass, bring all its least particles to an accurate polish; so that all their surfaces shall be truly plain or truly spherical, and look all the same way, so as together to compose one even surface. The smaller the particles of those substances are, the smaller will be the scratches, by which they continually fret and wear away the glass until it be polished; but, be they never so small, they can wear away the glass no otherwise, than by grating and scratching it, and breaking the protuberances; and therefore polish it no otherwise, than by bringing its roughness to a very fine grain, so that the scratches and frettings of the surface become too small to be visible. And therefore if light were reflected by impinging upon the solid Parts of the glass, it would be scattered as much by the most polished glass as by the roughest. So then it remains a Problem, how glass, polished by fretting substances, can reflect light so regularly as it does. And this Problem is scarce otherwise to be solved, than by saying, that the reflexion of a ray is effected, not by a single point of the reflecting body, but by some Power of the body which is evenly diffused all over its surface, and by which it acts upon the ray without immediate contact. For that the Parts of bodies do act upon light at a distance shall be shewn hereafter.

Now if light be reflected not by impinging on the solid Parts of bodies, but by some other principle; it is probable that as many of its rays as impinge on the solid Parts of bodies are not reflected, but stifled and lost in the bodies. For otherwise we must allow two sorts of reflexions. Should all the rays be reflected

reflected which impinge on the internal parts of clear water or crystal, those substances would rather have a cloudy colour than a clear transparency. To make bodies look Black, it is necessary that many rays be stopped, retained and lost in them; and it seems not probable, that any rays can be stopped and stifled in them, which do not impinge on their Parts.

And hence we may understand that bodies are much more rare and porous, than is commonly believed. Water is nineteen times lighter, and by consequence nineteen times rarer, than gold; and gold is so rare as very readily, and without the least opposition, to transmit the Magnetic effluvia, and easily to admit quick-silver into its pores, and to let water pass through it. For a concave sphere of gold filled with water, and soldered up, has, upon pressing the sphere with great force, let the water squeeze through it, and stand all over its outside in multitudes of small drops, like dew, without bursting or cracking the body of the gold; as I have been informed by an eye-witness. From all which we may conclude, that gold has more pores than solid parts, and by consequence that water has above forty times more Pores than Parts. And he that shall find out an hypothesis, by which water may be so rare, and yet not be capable of compression by force; may doubtless, by the same hypothesis, make gold and water, and all other bodies, as much rarer as he pleases, so that light may find a ready passage through transparent substances.

The magnet acts upon iron through all dense bodies not magnetic nor red hot, without any diminution of its virtue; as for instance through gold, silver, lead, glass, water. The Gravitating power of the sun is transmitted through the vast bodies of the planets without any diminution; so as to act upon all their parts to their very centers, with the same force, and according to the same laws, as if the part, upon which it acts, were not surrounded with the body of the planet. The rays of light, whether they be very small bodies projected, or only motion or force propagated, are moved in right lines; and whenever a ray of light is, by any obstacle, turned out of its rectilinear way, it will never return into the same rectilinear way, unless perhaps by very great accident. And yet light is transmitted through pellucid solid bodies

Porousness of Bodies. Bodies in right lines to very great distances. How bodies can have a sufficient quantity of pores for producing these effects, is very difficult to conceive; but perhaps not altogether impossible. For the colours of bodies arise from the magnitudes of the particles which reflect them, as was explained above. Now if we conceive these particles of bodies to be so disposed amongst themselves, that the intervals, or empty spaces between them, may be equal in magnitude to them all; and that these particles may be composed of other particles much smaller, which have as much empty space between them as equals all the magnitudes of these smaller particles; and that in like manner these smaller particles are again composed of others much smaller, all which together are equal to all the pores, or empty spaces, between them; and so on perpetually till you come to solid particles, such as have no pores, or empty spaces within them: and if in any gross body there be, for instance, three such degrees of particles, the least of which are solid; this body will have seven times more pores than solid parts. But if there be four such degrees of particles, the least of which are solid, the body will have fifteen times more pores than solid parts. If there be five degrees, the body will have one and thirty times more pores than solid parts. If six degrees, the body will have sixty and three times more pores than solid parts. And so on perpetually. And there are other ways of conceiving how bodies may be exceeding porous. But what is really their inward frame is not yet known to us.

P R O P. IX.

Bodies Reflect and Refract light by one and the same Power variously exercised in various circumstances.

Reflexion and Refraction

This appears by several considerations. First, Because when light goes out of glass into air, as obliquely as it can possibly do: if its incidence be made still more oblique, it becomes totally Reflected. For the power of the glass, after it has refracted the light as obliquely as is possible, if the incidence be still made more oblique, becomes too strong to let any of its rays go through, and by consequence causes total reflexions. Secondly, Because

Because light is alternately reflected and transmitted by thin plates of glass for many successions, accordingly as the thickness of the plate increases in an arithmetical progression. For here the thickness of the glass determines whether that power, by which glass acts upon light, shall cause it to be reflected, or suffer it to be transmitted. And, Thirdly, Because those surfaces of transparent bodies, which have the greatest Refracting power, reflect the greatest quantity of light, as was shewed in the first Proposition.

P R O P. X.

If light be swifter in bodies than in vacuo, in the proportion of the sines which measure the refraction of the bodies; the forces of the bodies to reflect and refract light, are very nearly proportional to the Densities of the same bodies, excepting that Unctuous and Sulphureous bodies refract more than others of this same density.

Let AB represent the refracting plane surface of any body, and IC a ray incident very obliquely upon the body in C, so that the angle ACI may be infinitely little; and let CR be the refracted ray. From a given point, B, perpendicular to the refracting surface erect BR, meeting with the refracted ray, CR, in R; and if CR represent the motion of the refracted ray, and this motion be

distinguished into two motions, CB and BR; whereof CB is parallel to the refracting plane, and BR perpendicular to it: CB shall represent the motion of the incident ray, and BR the motion generated by the refraction, as Opticians have of late explained.

Now if any body or thing, in moving through any space of a given breadth terminated on both sides by two parallel planes, be urged forward, in all parts of that space, by forces tending directly forwards towards the last plane, and before its incidence on the first plane, had no motion towards it, or but an infinitely little one; and if the forces, in all parts of that space, between

Refractive and
Reflective
force

the planes, be at equal distances from the planes equal to one another; but at several distances be bigger or less in any given proportion: the motion generated by the forces, in the whole passage of the body or thing through that space, shall be in a subduplicate proportion of the forces, as Mathematicians will easily understand (9). And therefore if the space of Activity of the refracting superficies of the body be considered as such a space, the motion of the ray generated by the Refracting force of the body, during its passage through that space, that is the motion BR, must be in a subduplicate proportion of that Refracting force. I say, therefore, that the square of the line BR, and by consequence the Refracting force of the body, is very nearly as the Density of the same body. For this will appear by the following Table, wherein the proportion of the sines which measure the refractions of several bodies, the square of BR, supposing CB an unit, the densities of the bodies estimated by their specific Gravities, and their Refractive power in respect of their Densities, are set down in several columns.

(9) See above, Book I. Part I. Ex. 15. Not. 7.

The

proportional
to Density.

The refracting bodies.	The proportion of the sines of incidence and refraction of yellow light.	The square of BR, to which the refracting force of the body is proportionate.	The density and specific gravity of the body.	The refractive power of the body in respect of its density.
A Pseudo-Topazius, being a natural, pellucid, brittle, hairy stone, of a Yellow colour.	23 to 14	1'699	4'27	3979
Air.	3201 to 3200	0'000625	0'0012	5208
Glass of Antimony.	17 to 9	2'568	5'28	4864
A Selenitis.	61 to 41	1'213	2'252	5386
Glass vulgar.	31 to 20	1'4025	2'58	5436
Crystal of the Rock.	25 to 16	1'445	2'65	5450
Island Crystal.	5 to 3	1'778	2'72	6536
Sal Gemmæ.	17 to 11	1'388	2'143	6477
Alume.	35 to 24	1'1267	1'714	6570
Borax.	22 to 15	1'1511	1'714	6716
Niter.	32 to 21	1'345	1'9	7079
Dantzick Vitriol.	303 to 200	1'295	1'715	7551
Oil of Vitriol.	10 to 7	1'041	1'7	6124
Rain Water.	529 to 396	0'7845	1'	7845
Gum Arabick.	31 to 21	1'179	1'375	8574
Spirit of Wine well rectified.	100 to 73	0'8765	0'866	10121
Camphire.	3 to 2	1'25	0'996	12551
Oil Olive.	22 to 15	1'1511	0'913	12607
Linseed Oil.	40 to 27	1'1948	0'932	12819
Spirit of Turpentine.	25 to 17	1'1626	0'874	13222
Ambar.	14 to 9	1'42	1'04	13654
A Diamond.	100 to 41	4'949	3'4	14556

The refraction of the air, in this Table, is determined by that of the atmosphere observed by Astronomers. For if light pass through many refracting substances or mediums, gradually denser and denser, and terminated with parallel surfaces, the sum of all the refractions will be equal to the single refraction, which it would have suffered in passing immediately out of the first medium into the last. And this holds true, though the number of the refracting substances be increased to infinity, and the distances from one another as much decreased; so that the light may be refracted in every point of its passage, and, by continual refractions, bent into a curve line. And therefore the whole refraction of light, in passing through the atmosphere from the highest and rarest part thereof down to the lowest and densest part, must be equal to the refraction which it would suffer in passing, at like obliquity, out of a vacuum immediately into air, of equal density with that in the lowest part of the atmosphere.

Now

Now although a Pseudo-topaz, a Selenitis, Rock Crystal, Island Crystal, Vulgar Glass (that is, sand melted together) and glass of Antimony, which are terrestrial stony alcalizate Concretes, and air, which probably arises from such substances by fermentation, be substances very differing from one another in Density; yet by this Table, they have their Refractive powers almost in the same proportion to one another as their Densities are, excepting that the refraction of that strange substance Island Crystal is a little bigger than the rest. And particularly air, which is 3500 times rarer than the Pseudo-Topaz, and 4400 times rarer than Glass of Antimony, and 2000 times rarer than the Selenitis, Glass Vulgar, or Crystal of the Rock, has, notwithstanding its rarity, the same Refractive power in respect of its Density, which the very dense substances have in respect of theirs, excepting so far as those differ from one another.

Again, the refraction of Camphire, Oil Olive, Linseed Oil, Spirit of Turpentine and Ambar, which are fat sulphureous unctuous bodies, and a Diamond, which probably is an unctuous substance coagulated, have their Refractive Powers in proportion to one another as their Densities, without any considerable variation. But the Refractive Powers of these unctuous substances are two or three times greater, in respect of their Densities, than the Refractive Powers of the former substances in respect of theirs.

Water has a Refractive Power in a middle degree between those two sorts of substances, and probably is of a middle nature. For out of it grow all Vegetable and Animal substances, which consist as well of sulphureous fat and inflammable parts, as of earthly lean and alcalizate ones.

Salts and Vitriols have Refractive Powers in a middle degree between those of Earthly substances and water; and accordingly are composed of those two sorts of substances. For by distillation and rectification of their spirits a great part of them goes into water, and a great part remains behind in the form of a dry fixed earth, capable of vitrification.

Spirit of Wine has a Refractive Power in a middle degree between those of Water and Oily substances; and accordingly seems to

to be composed of both, united by fermentation; the water, by means of some saline spirits with which it is impregnated, dissolving the oil, and volatilizing it by the action. For Spirit of Wine is inflammable by means of its oily parts; and being distilled often from Salt of Tartar, grows by every distillation more and more Aqueous and Phlegmatick. And Chemists observe, that Vegetables (as Lavender, Rue, Marjoram, &c.) distilled *per se*, before fermentation yield oils without any burning spirits; but after fermentation, yield ardent spirits without oils: which shews, that their oil is by fermentation converted into spirit. They find also, that if oils be poured in small quantity upon fermentating vegetables, they distil over after fermentation in the form of spirits.

So then, by the foregoing Table, all bodies seem to have their Refractive Powers proportional to their Densities, or very nearly; excepting so far as they partake more or less of sulphureous oily particles, and thereby have their Refractive power made greater or less. Whence it seems rational to attribute the Refractive power of all bodies chiefly, if not wholly, to the sulphureous parts with which they abound. For it is probable that all bodies abound more or less with sulphurs. And as light, congregated by a burning-glass, acts most upon sulphureous bodies, to turn them into fire and flame; so, since all action is mutual, sulphurs ought to act most upon light. For that the action between light and bodies is mutual, may appear from this consideration; that the densest bodies, which refract and reflect light most strongly, grow hottest in the summer-sun, by the action of the refracted or reflected light.

I have hitherto explained the power of bodies to reflect and refract; and shewed, that thin transparent plates, fibres and particles do, according to their several thickneses and densities, reflect several sorts of rays, and thereby appear of several colours; and, by consequence, that nothing more is requisite for producing all the colours of Natural Bodies, than the several sizes and densities of their transparent particles. But whence it is that these plates, fibres and particles do, according to their several thickneses and densities, reflect several sorts of rays, I have not yet explained.

explained. To give some insight into this matter, and make way for understanding the next part of this Book, I shall conclude this Part with a few more Propositions. Those, which preceded, respect the nature of bodies: these, the nature of light: for both must be understood, before the reason of their actions upon one another can be known. And because the last Proposition depended upon the velocity of light, I will begin with a Proposition of that kind.

P R O P. XI.

Light is propagated from luminous Bodies in time; and spends about seven or eight minutes of an hour in passing from the Sun to the Earth.

Light propagated in time.

This was observed first by *Roemer*, and then by others, by means of the eclipses of the satellites of *Jupiter*. For these eclipses, when the earth is between the Sun and *Jupiter*, happen about seven or eight minutes sooner, than they ought to do by the Tables; and when the Earth is beyond the Sun, they happen about seven or eight minutes later than they ought to do; the reason being, that the light of the satellites has farther to go in the latter case, than in the former, by the diameter of the Earth's orbit. Some inequalities of time may arise from the excentricities of the orbs of the satellites; but those cannot answer in all the satellites, and at all times, to the position and distance of the Earth from the Sun. The mean motions of *Jupiter's* satellites is also swifter in his descent from his aphelium to his perihelium, than in his ascent in the other half of his orb: but this inequality has no respect to the position of the Earth, and in the three interior satellites is insensible; as I find by computation from the theory of their gravity.

P R O P.

P R O P. XII.

Every ray of Light, in its passage through any refracting surface, is put into a certain transient constitution or state; which, in the progress of the ray, returns at equal intervals; and disposes the ray, at every return, to be easily transmitted through the next refracting surface, and, between the returns, to be easily reflected by it.

This is manifest by the 5th, 9th, 12th and 15th Observations. Fits of Transmission and Reflexion. For by those Observations it appears, that one and the same sort of rays, at equal angles of incidence on any thin transparent plate, is alternately reflected and transmitted for many successions, accordingly as the Thickness of the plate increases in arithmetical progression of the numbers, 0, 1, 2, 3, 4, 5, 6, 7, 8, &c. so that if the First reflexion (that which makes the first or innermost of the rings of colours there described) be made at the thickness 1, the rays shall be transmitted at the thicknesses 0, 2, 4, 6, 8, 10, 12, &c. and thereby make the central spot and rings of light, which appear by transmission; and be reflected at the thickness 1, 3, 5, 7, 9, 11, &c. and thereby make the rings which appear by reflexion. And this alternate reflexion and transmission, as I gather by the 24th Observation, continues for above an hundred vicissitudes, and by the Observations in the next part of this Book, for many thousands; being propagated from one surface of a glass-plate to the other, though the thickness of the plate be a quarter of an inch, or above; so that this alternation seems to be propagated from every refracting surface, to all distances, without end or limitation.

This alternate reflexion and refraction depends on both the surfaces of every thin plate, because it depends on their distance. By the 21st Observation, if either surface of a thin plate of Muscovy-glass be wetted, the colours caused by the alternate reflexion and refraction grow faint, and therefore it depends on them both.

It is therefore performed at the second surface; for if it were performed at the first, before the rays arrive at the second, it would not depend on the second.

Fits of
Transmission

It is also influenced by some action or disposition, propagated from the first to the second; because otherwise at the second it would not depend on the first. And this action or disposition, in its propagation, intermits and returns by equal intervals; because in all its progress it inclines the ray, at one distance from the first surface, to be reflected by the second; at another, to be transmitted by it, and that by equal intervals for innumerable vicissitudes. And because the ray is disposed to reflexion at the distances 1, 3, 5, 7, 9, &c. and to transmission at the distances 0, 2, 4, 6, 8, 10, &c. (for its transmission through the first surface is at the distance 0, and it is transmitted through both together, if their distance be infinitely little or much less than 1) the disposition to be transmitted at the distances 2, 4, 6, 8, 10, &c. is to be accounted a return of the same disposition which the ray first had at the distance 0; that is, at its transmission through the first refracting surface. All which is the thing I would prove.

What kind of action or disposition this is; whether it consists in a circulating or a vibrating motion of the ray, or of the medium, or something else, I do not here enquire. Those that are averse from assenting to any new discoveries, but such as they can explain by an hypothesis, may for the present suppose, that as stones, by falling upon water, put the water into an undulating motion, and all bodies by percussion excite vibrations in the air; so the rays of light, by impinging on any refracting or reflecting surface, excite vibrations in the refracting or reflecting medium or substance, and by exciting them agitate the solid parts of the refracting or reflecting body, and by agitating them cause the body to grow warm or hot; that the vibrations, thus excited, are propagated in the refracting or reflecting medium or substance, much after the manner that vibrations are propagated in the air for causing sound, and move faster than the rays, so as to overtake them; and that when any ray is in that part of the vibration which conspires with its motion, it easily breaks through a reflecting surface; but when it is in the contrary part of the vibration which impedes its motion, it is easily reflected; and, by consequence, that every ray is successively disposed to be easily reflected or easily transmitted, by every vibration which overtakes

takes it. But whether this hypothesis be true or false, I do not here consider. I content myself with the bare discovery, that the rays of light are, by some cause or other, alternately disposed to be reflected or refracted for many vicissitudes.

D E F I N I T I O N.

The returns of the disposition of any Ray to be reflected I will call its Fits of easy Reflexion; and those of its disposition to be transmitted, its Fits of easy Transmission; and the space it passes between every return and the next return, the Interval of its Fits.

P R O P. XIII.

The reason why the surfaces of all thick transparent bodies Reflect part of the light incident on them, and Refract the rest, is, that some rays, at their incidence, are in Fits of easy Reflexion, and others in Fits of easy Transmission.

This may be gathered from the 24th Observation, where the light reflected by thin plates of air and glass, which to the naked eye appeared evenly white all over the plate, did through a prism appear waved with many successions of light and darkness, made by alternate fits of easy Reflexion and easy Transmission, the prism severing and distinguishing the waves of which the White reflected light was composed, as was explained above.

And hence light is in fits of easy Reflexion and easy Transmission, before its incidence on transparent bodies. And probably it is put into such fits at its first emission from luminous bodies, and continues in them during all its progress. For these fits are of a lasting nature, as will appear by the next part of this Book.

In this Proposition I suppose the transparent bodies to be thick; because if the thickness of the body be much less than the interval of the fits of easy Reflexion and Transmission of the rays, the body loseth its reflecting power. For if the rays, which at their entering into the body are put into fits of easy Transmission, arrive at the farthest surface of the body before they be out of those fits, they must be transmitted. And this is the reason

Fits of Trans-
mission

son why bubbles of water lose their reflecting power when they grow very thin, and why all Opaque bodies, when reduced into very small parts, become transparent.

P R O P. XIV.

The surfaces of transparent bodies, which, if the ray be in a fit of Refraction, do refract it most strongly; if the ray be in a fit of Reflexion, do reflect it most easily.

For we shewed above in Prop. VIII. that the cause of Reflexion is not the impinging of light on the solid impervious Parts of bodies, but some other power, by which those solid Parts act on light at a distance. We shewed also in Prop. IX. that bodies reflect and refract light by one and the same power variously exercised in various circumstances; and in Prop. I. that the most strongly refracting surfaces reflect the most light: all which compared together evince and ratify both this and the last Proposition.

P R O P. XV.

In any one and the same sort of rays, emerging in any angle out of any refracting surface into one and the same medium, the Intervals of the following fits of easy Reflexion and Transmission are, either accurately or very nearly, as the rectangle of the secant of the angle of refraction, and of the secant of another angle, whose sine is the first of 106 arithmetical mean proportionals between the sines of incidence and refraction, counted from the sine of refraction.

This is manifest by the 7th and 19th Observations.

P R O P.

P R O P. XVI.

and
Reflexion.

In several sorts of rays, emerging in equal angles out of any refracting surface into the same medium, the intervals of the following fits of easy Reflexion and easy Transmission are, either accurately or very nearly, as the cube-roots of the squares of the lengths of a chord, which sound the notes of an eight, sol, la, fa, sol, la, mi, fa, sol, with all their intermediate degrees answering to the colours of those rays, according to the analogy described in the seventh Experiment of the second Part of the first Book.

This is manifest by the 13th and 14th Observations.

P R O P. XVII.

If rays of any sort pass perpendicularly into several mediums, the intervals of the fits of easy Refraction and Transmission in any one medium, are to those intervals in any other, as the sine of incidence to the sine of refraction, when the rays pass out of the first of those two mediums into the second.

This is manifest by the 10th Observation.

P R O P. XVIII.

If the rays, which paint the colour in the confine of Yellow and Orange, pass perpendicularly out of any medium into air, the intervals of their fits of easy Reflexion are the $\frac{1}{87000}$ th part of an inch. And of the same length are the intervals of their fits of easy Transmission.

This is manifest by the 6th Observation.

From these Propositions it is easy to collect the intervals of the fits of easy Reflexion and Transmission of any sort of rays, refracted in any angle into any medium; and thence to know, whether the rays shall be reflected or transmitted, at their subsequent incidence upon any other pellucid medium. Which thing, being useful for understanding the next part of this Book, was here to be set down. And for the same reason I add the two following Propositions.

P R O P.

P R O P. XIX.

Fits of
Transmission

If any sort of rays, falling on the polite surface of any pellucid medium, be reflected back; the fits of easy Reflexion, which they have at the point of reflexion, shall still continue to return; and the returns shall be at distances from the point of reflexion in the arithmetical progression of the numbers 2, 4, 6, 8, 10, 12, &c. and between these fits the rays shall be in fits of easy Transmission.

For since the fits of easy Reflexion and easy Transmission are of a returning nature, there is no reason why these fits, which continued till the ray arrived at the reflecting medium, and there inclined the ray to reflexion, should there cease. And if the ray, at the point of reflexion, was in a fit of easy reflexion, the progression of the distances of these fits from that point must begin from 0, and so be of the numbers 0, 2, 4, 6, 8, &c. And therefore the progression of the distances of the intermediate fits of easy Transmission, reckoned from the same point, must be in the progression of the odd numbers 1, 3, 5, 7, 9, &c. contrary to what happens, when the fits are propagated from points of refraction.

P R O P. XX.

The intervals of the fits of easy Reflexion and easy Transmission, propagated from points of reflexion into any medium, are equal to the intervals of the like fits, which the same rays would have, if refracted into the same medium in angles of refraction equal to their angles of reflexion.

For when light is reflected by the second surface of thin plates, it goes out afterwards freely at the first surface, to make the rings of colours which appear by reflexion; and by the freedom of its egress, makes the colours of these rings more vivid and strong, than those which appear on the other side of the plates, by the transmitted light. The reflected rays are therefore in fits of easy Transmission at their egress; which would not always happen, if the intervals of the fits within the plate, after reflexion, were not

not equal both in length and number to their intervals before it. ^{and Reflexion.} And this confirms also the proportions set down in the former Proposition. For if the rays, both in going in and out at the first surface, be in fits of easy Transmission, and the intervals and numbers of those fits between the first and second surface, before and after reflexion, be equal; the distances of the fits of easy Transmission from either surface must be in the same progression after reflexion as before; that is, from the First surface which transmitted them, in the progression of the Even numbers 0, 2, 4, 6, 8, &c. and from the Second which reflected them, in that of the Odd numbers 1, 3, 5, 7, &c. But these two Propositions will become much more evident by the Observations in the following part of this Book.

T H E

T H E
S E C O N D B O O K
O F
O P T I C K S.

P A R T IV.

Observations concerning the Reflexions and Colours of thick transparent polished Plates.

Reflexion and
Colours

THERE is no glass or speculum, how well soever polished, but, besides the light which it refracts or reflects regularly, scatters every way irregularly a faint light; by means of which the polished surface, when illuminated in a dark room by a beam of the sun's light, may be easily seen in all positions of the eye. There are certain phenomena of this scattered light, which when I first observed them, seemed very strange and surprising to me. My Observations were as follows.

Obs. 1. The sun shining into my darkened chamber, through a hole one-third of an inch wide, I let the intronitted beam of light fall perpendicularly upon a glass-speculum, ground concave on one side and convex on the other, to a sphere of five feet and eleven inches radius, and quick-silvered over on the convex side. And holding a white opaque chart or a quire of paper, at the center of the spheres to which the speculum was ground, that is, at the distance of about five feet and eleven inches from the speculum, in such manner, that the beam of light might pass through

through a little hole made in the middle of the chart to the speculum, and thence be reflected back to the same hole: I observed upon the chart four or five concentric irises, or rings of colours, like rainbows, encompassing the hole; much after the manner that those, which in the fourth and following Observations of the first Part of the third (1) Book appeared between the object-glasses, encompassed the Black spot, but yet larger and fainter than those. These rings, as they grew larger and larger, became diluter and fainter, so that the fifth was scarce visible. Yet sometimes, when the sun shone very clear, there appeared faint lineaments of a sixth and seventh. If the distance of the chart from the speculum was much greater or much less than that of six feet, the rings became dilute and vanished. And if the distance of the speculum from the window was much greater than that of six feet, the reflected beam of light would be so broad at the distance of six feet from the speculum, where the rings appeared, as to obscure one or two of the innermost rings. And therefore I usually placed the speculum at about six feet from the window; so that its focus might there fall in with the center of its concavity, at the rings upon the chart. And this posture is always to be understood in the following Observations where no other is expressed.

Obs. 2. The colours of these rainbows succeeded one another from the center outwards, in the same form and order, with those which were made in the ninth Observation of the first Part of this Book by light not reflected, but transmitted through the two object-glasses. For, first, there was in their common center a White round spot of faint light, something broader than the reflected beam of light; which beam sometimes fell upon the middle of the spot, and sometimes, by a little inclination of the speculum, receded from the middle, and left the spot white to the center.

This White spot was immediately encompassed with a dark Grey or Russet, and that dark Grey with the colours of the first iris; which colours, on the inside next the dark Grey, were a little Violet and Indigo, and next to that a Blue, which on the

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Colours

outside grew pale; and then succeeded a little greenish Yellow; and after that a brighter Yellow; and then, on the outward edge of the iris, a Red, which on the outside inclined to Purple.

This iris was immediately encompassed with a second; whose colours were in order from the inside outwards, Purple, Blue, Green, Yellow, light Red, a Red mixed with Purple.

Then immediately followed the colours of the third iris; which were in order outwards a Green inclining to Purple, a good Green, and a Red more bright than that of the former iris.

The fourth and fifth iris seemed of a blueish Green within, and Red without; but so faintly that it was difficult to discern the colours.

Obs. 3. Measuring the diameters of these rings upon the chart, as accurately as I could, I found them also in the same proportion to one another with the rings made by light transmitted through the two object-glasses. For the diameters of the four first of the bright rings, measured between the brightest parts of their orbits, at the distance of six feet from the speculum, were $1\frac{1}{16}$, $2\frac{3}{8}$, $2\frac{1}{2}$, $3\frac{3}{8}$ inches; whose squares are in arithmetical progression of the numbers 1, 2, 3, 4. If the White circular spot in the middle be reckoned amongst the rings, and its central light, where it seems to be most luminous, be put equipollent to an infinitely little ring; the squares of the diameters of the rings will be in the progression 0, 1, 2, 3, 4, &c. I measured also the diameters of the dark circles between these luminous ones; and found their squares in the progression of the numbers $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, &c. the diameters of the first four, at the distance of six feet from the speculum, being $1\frac{3}{16}$, $2\frac{1}{16}$, $2\frac{2}{3}$, $4\frac{3}{16}$ (⁵) inches. If the distance of the chart from the speculum was increased or diminished, the diameters of the circles were increased or diminished proportionally.

Obs. 4. By the analogy between these rings and those described in the Observations of the first Part of this Book, I suspected that there were many more of them which spread into one another, and by interfering mixed their colours, and diluted one another so that they could not be seen apart. I viewed them therefore

(⁵) $4\frac{3}{16}$. So the third edition. All the rest $3\frac{3}{16}$.

through

through a prism, as I did those in the 24th Observation of the first Part of this Book. And when the prism was so placed as, by refracting the light of their mixed colours, to separate them, and distinguish the rings from one another, as it did those in that Observation; I could then see them distincter than before, and easily number eight or nine of them, and sometimes twelve or thirteen. And had not their light been so very faint, I question not but that I might have seen many more.

Obs. 5. Placing a prism at the window to refract the intromitted beam of light, and cast the oblong spectrum of colours on the speculum: I covered the speculum with a black paper, which had in the middle of it a hole to let any one of the colours pass through to the speculum, whilst the rest were intercepted by the paper. And now I found rings of that colour only, which fell upon the speculum. If the speculum was illuminated with Red, the rings were totally Red with dark intervals; if with Blue, they were totally Blue, and so of the other colours. And when they were illuminated with any one colour, the squares of their diameters, measured between their most luminous parts, were in the arithmetical progression of the numbers 0, 1, 2, 3, 4; and the squares of the diameters of their dark intervals, in the progression of the intermediate numbers $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$. But if the colour was varied, they varied their magnitude. In the Red, they were largest; in the Indigo and Violet, least; and in the intermediate colours, Yellow, Green and Blue, they were of several intermediate bignesses answering to the colour; that is, greater in Yellow than in Green, and greater in Green than in Blue. And hence I knew, that when the speculum was illuminated with White light, the Red and Yellow, on the outside of the rings, were produced by the least refrangible rays, and the Blue and Violet by the most refrangible; and that the colours of each ring spread into the colours of the neighbouring rings on either side, after the manner explained in the first and second Part of this Book; and, by mixing, diluted one another so that they could not be distinguished; unless near the center, where they were least mixed. For in this Observation I could see the rings more distinctly, and to a greater number than before; being able in the Yellow light to

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Colours

number eight or nine of them, besides a faint shadow of a tenth. To satisfy myself how much the colours of the several rings spread into one another, I measured the diameters of the second and third rings; and found them, when made by the confine of the Red and Orange, to be to the same diameters, when made by the confine of Blue and Indigo, as 9 to 8, or thereabouts. For it was hard to determine this proportion accurately. Also the circles made successively by the Red, Yellow and Green, differed more from one another than those made successively by the Green, Blue and Indigo. For the circle made by the Violet was too dark to be seen. To carry on the computation, let us therefore suppose, that the differences of the diameters of the circles made by the utmost Red, the confine of Red and Orange, the confine of Orange and Yellow, the confine of Yellow and Green, the confine of Green and Blue, the confine of Blue and Indigo, the confine of Indigo and Violet, and outmost Violet, are in proportion as the differences of the lengths of a monochord which sound the tones in an eight; *sol, la, fa, sol, la, mi, fa, sol*, that is, as the numbers $\frac{1}{9}, \frac{1}{8}, \frac{1}{12}, \frac{1}{12}, \frac{1}{27}, \frac{1}{27}, \frac{1}{18}$. And if the diameter of the circle made by the confine of Red and Orange be $9A$, and that of the circle made by the confine of Blue and Indigo be $8A$, as above; their difference, $9A - 8A$, will be to the difference of the diameters of the circle made by the outmost Red, and by the confine of Red and Orange as $\frac{1}{18} + \frac{1}{12} + \frac{1}{12} + \frac{1}{27}$ to $\frac{1}{9}$, that is as $\frac{8}{27}$ to $\frac{1}{9}$, or 8 to 3; and to the difference of the circles made by the outmost Violet, and by the confine of Blue and Indigo as $\frac{1}{18} + \frac{1}{12} + \frac{1}{12} + \frac{1}{27}$ to $\frac{1}{27} + \frac{1}{18}$, that is, as $\frac{8}{27}$ to $\frac{5}{54}$, or as 16 to 5. And therefore these differences will be $\frac{3}{8}A$ and $\frac{6}{16}A$. Add the first to $9A$, and subtract the last from $8A$, and you will have the diameters of the circles made by the least and most refrangible rays, $\frac{75}{8}A$ and $\frac{61}{8}A$. These diameters are therefore to one another as 75 to 61, or 50 to 41; and their squares as 2500 to 1681, that is, as 3 to 2 very nearly. Which proportion differs not much from the proportion of the diameters of the circles made by the outmost Red and outmost Violet, in the 13th Observation of the first Part of this Book.

Obs. 6. Placing my eye where these rings appeared plainest, I saw the speculum tinged all over with waves of colours (Red, Yel-

low, Green, Blue); like those which, in the Observations of the first Part of this Book, appeared between the object-glasses and upon bubbles of water, but much larger. And after the manner of those, they were of various magnitudes in various positions of the eye; swelling and shrinking as I moved my eye this way and that way. They were formed like arcs of concentric circles, as those were; and when my eye was over against the center of the concavity of the speculum (that is, 5 feet and 10 inches distant from the speculum) their common center was in a right line with that center of concavity, and with the hole in the window. But in other postures of my eye their center had other positions. They appeared by the light of the clouds, propagated to the speculum through the hole in the window; and when the sun shone through that hole upon the speculum, his light upon it was of the colour of the ring whereon it fell; but, by its splendor, obscured the rings made by the light of the clouds, unless when the speculum was removed to a great distance from the window, so that his light upon it might be broad and faint. By varying the position of my eye, and moving it nearer to or farther from the direct beam of the sun's light, the colour of the sun's reflected light constantly varied upon the speculum, as it did upon my eye; the same colour always appearing to a by-stander upon my eye, which to me appeared upon the speculum. And thence I knew, that the rings of colours upon the chart were made by these reflected colours, propagated thither from the speculum in several angles; and that their production depended not upon the termination of light and shadow.

Obs. 7. By the analogy of all these phenomena with those of the like rings of colours described in the first Part of this Book; it seemed to me, that these colours were produced by this thick plate of glass, much after the manner that those were produced by very thin plates. For, upon trial, I found that if the quick-silver were rubbed off from the backside of the speculum, the glass alone would cause the same rings of colours; but much more faint than before: and therefore the phenomenon depends not upon the quick-silver, unless so far as the quick-silver, by increasing the reflexion of the backside of the glass, increases the light.

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light of the rings of colours. I found also that a speculum of metal without glass, made some years since for optical uses, and very well wrought, produced none of those rings; and thence I understood that these rings arise not from one specular surface alone, but depend upon the two surfaces of the plate of glass whereof the speculum was made, and upon the thickness of the glass between them. For, as in the 7th and 19th Observations of the first Part of this Book, a thin plate of air, water or glass, of an even thickness, appeared of one colour, when the rays were perpendicular to it; of another, when they were a little oblique; of another, when more oblique; of another, when still more oblique, and so on: so here, in the sixth Observation, the light, which emerged out of the glass in several obliquities, made the glass appear of several colours; and being propagated in those obliquities to the chart, there painted rings of those colours. And as the reason why a thin plate appeared of several colours in several obliquities of the rays, was, that the rays of one and the same sort are reflected by the thin plate at one obliquity, and transmitted at another; and those of other sorts transmitted where these are reflected, and reflected where these are transmitted: so the reason why the thick plate of glass, whereof the speculum was made, did appear of various colours in various obliquities, and in those obliquities propagated those colours to the chart, was, that the rays of one and the same sort did, at one obliquity, emerge out of the glass; at another, did not emerge, but were reflected back towards the quick-silver by the hither surface of the glass: and, accordingly as the obliquity became greater and greater, emerged and were reflected alternately for many successions; and that in one and the same obliquity the rays of one sort were reflected, and those of another transmitted. This is manifest by the fifth Observation of this Part of this Book. For in that Observation, when the speculum was illuminated by any one of the Prismatic Colours, that light made many rings of the same colour upon the chart with dark intervals; and therefore, at its emergence out of the speculum, was alternately transmitted and not transmitted from the speculum to the chart for many successions, according to the various obliquities of its emergence.

of thick trans-
parent Plates.

ence. And when the colour cast on the speculum by the prism was varied, the rings became of the colour cast on it, and varied their bigness with their colour; and therefore the light was now alternately transmitted and not transmitted, from the speculum to the chart, at other obliquities than before. It seemed to me therefore, that these rings were of one and the same original with those of thin plates; but yet with this difference, that those of thin plates are made by the alternate reflexions and transmissions of the rays at the second surface of the plate, after one passage through it; but here, the rays go twice through the plate, before they are alternately reflected and transmitted. First, they go through it from the first surface to the quick-silver; and then return through it from the quick-silver to the first surface; and there are either transmitted to the chart, or reflected back to the quick-silver, accordingly as they are in their fits of easy Reflexion or Transmission, when they arrive at that surface. For the intervals of the fits of the rays, which fall perpendicularly on the speculum and are reflected back in the same perpendicular lines, by reason of the equality of these angles and lines, are of the same length and number within the glass after reflexion, as before; by the 19th Proposition of the third Part of this Book. And therefore, since all the rays that enter through the first surface are in their fits of easy Transmission at their entrance; and as many of these as are reflected by the second, are in their fits of easy Reflexion there; all these must be again in their fits of easy Transmission at their return to the first; and by consequence there go out of the glass to the chart, and form upon it the White spot of light in the center of the rings. For the reason holds good in all sorts of rays, and therefore all sorts must go out promiscuously to that spot, and by their mixture cause it to be White. But the intervals of the fits of those rays, which are reflected more obliquely than they enter, must be greater after reflexion than before, by the 15th and 20th Propositions. And hence it may happen that the rays, at their return to the first surface, may, in certain obliquities, be in fits of easy Reflexion, and return back to the quick-silver; and, in other intermediate obliquities, be again in fits of easy Transmission, and so go out to the chart, and paint on it

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it the rings of colours about the White spot. And because the intervals of the fits, at equal obliquities, are greater and fewer in the less refrangible rays, and less and more numerous in the more refrangible; therefore the less refrangible, at equal obliquities, shall make fewer rings than the more refrangible; and the rings made by those shall be larger than the like number of rings made by these; that is, the Red rings shall be larger than the Yellow, the Yellow than the Green, the Green than the Blue, and the Blue than the Violet; as they were really found to be in the fifth Observation. And therefore the first ring, of all colours encompassing the White spot of light, shall be Red without any Violet within, and Yellow and Green and Blue in the middle, as it was found in the second Observation; and these colours in the second ring, and those that follow, shall be more expanded, till they spread into one another, and blend one another by interfering.

These seem to be the reasons of these rings in general; and this put me upon observing the Thickness of the glass, and considering whether the dimensions and proportions of the rings may be truly derived from it by computation.

Obs. 8. I measured therefore the thickness of this concavo-convex plate of glass, and found it every where $\frac{1}{4}$ of an inch precisely. Now, by the sixth Observation of the first Part of this Book, a thin plate of air transmits the brightest light of the First ring, that is the bright Yellow, when its thickness is the $\frac{1}{89000}$ th part of an inch; and by the tenth Observation of the same Part, a thin plate of glass transmits the same light of the same ring, when its thickness is less in proportion of the sine of refraction to the sine of incidence; that is, when its thickness is the $\frac{11}{1513000}$ th or $\frac{1}{137545}$ th part of an inch, supposing the sines are as 11 to 17. And if this thickness be doubled, it transmits the same bright light of the Second ring; if tripled, it transmits that of the Third, and so on; the bright Yellow light in all these cases being in its fits of Transmission. And therefore if its thickness be multiplied 34386 times, so as to become $\frac{1}{4}$ of an inch, it transmits the same bright light of the 34386th ring. Suppose this be the bright Yellow light, transmitted perpendicularly from the reflecting convex side of the glass through the concave side, to the

the White spot in the center of the rings of colours on the chart: of thick transparent Plates.
and by a rule in the 7th and 19th Observations in the first Part of this Book, and by the 15th and 20th Propositions of the third Part of this Book, if the rays be made oblique to the glass, the thickness of the glass requisite to transmit the same bright light of the same ring, in any obliquity, is to this thickness of $\frac{1}{4}$ of an inch, as the secant of a certain angle to the radius; the sine of which angle is the first of an hundred and six arithmetical means between the sines of incidence and refraction, counted from the sine of incidence, when the refraction is made out of any plated body into any medium encompassing it, that is, in this case, out of glass into air. Now if the thickness of the glass be increased by degrees, so as to bear to its first thickness (viz. that of a quarter of an inch) the proportions which 34386 (the number of fits of the Perpendicular rays in going through the glass towards the White spot in the center of the rings) hath to 34385, 34384, 34383 and 34382 (the numbers of the fits of the Oblique rays in going through the glass towards the first, second, third and fourth rings of colours). And if the first thickness be divided into 10000000 equal parts, the increased thicknesses will be 100002908, 100005816, 100008725 and 100011633; and the angles, of which these thicknesses are secants, will be 26' 13", 37' 5", 45' 6" and 52' 26", the radius being 100000000; and the sines of these angles are 762, 1079, 1321 and 1525; and the proportional sines of refraction 1172, 1659, 2031 and 2345, the radius being 100000. For since the sines of incidence, out of glass into air, are to the sines of refraction as 11 to 17; and to the above-mentioned secants, as 11 to the first of 106 arithmetical means between 11 and 17, that is, as 11 to $11\frac{6}{106}$; those secants will be to the sines of refraction, as $11\frac{6}{106}$ to 17, and by this analogy will give these sines. So then if the obliquities of the rays to the concave surface of the glass be such, that the sines of their refraction, in passing out of the glass through that surface into the air, be 1172, 1659, 2031, 2345, the bright light of the 34386th ring shall emerge at the thicknesses of the glass which are to $\frac{1}{4}$ of an inch, as 34386 to 34385, 34384, 34383, 34382, respectively. And therefore if the thickness

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thickness in all these cases be $\frac{1}{4}$ of an inch (as it is in the glass of which the speculum was made) the bright light of the 34385th ring shall emerge where the sine of refraction is 1172; and that of the 34384th, 34383d and 34382d ring, where the sine is 1659, 2031 and 2345 respectively. And in these angles of refraction the light of these rings shall be propagated from the speculum to the chart; and there paint rings about the white central round spot of light, which we said was the light of the 34386th ring. And the semi-diameters of these rings shall subtend the angles of refraction made at the concave surface of the speculum; and by consequence their diameters shall be to the distance of the chart from the speculum, as those sines of refraction doubled are to the radius; that is, as 1172, 1659, 2031, and 2345, doubled, are to 100000. And therefore if the distance of the chart from the concave surface of the speculum be six feet (as it was in the third of these Observations) the diameters of the rings of this bright Yellow light upon the chart shall be 1'688, 2'389, 2'925, 3'375 inches. For these diameters are to six feet, as the above-mentioned sines doubles are to the radius. Now these diameters of the bright Yellow rings, thus found by computation, are the very same with those found in the third of these Observations by measuring them, viz. with $1\frac{11}{16}$, $2\frac{3}{8}$, $2\frac{11}{12}$, and $3\frac{3}{8}$ inches; and therefore the theory of deriving these rings from the Thickness of the plate of glass, of which the speculum was made, and from the obliquity of the emerging rays, agrees with the Observation. In this computation I have equalled the diameters of the bright rings made by light of all colours, to the diameters of the rings made by the bright Yellow. For this Yellow makes the brightest part of the rings of all colours. If you desire the diameters of the rings made by the light of any other unmixed colour, you may find them readily, by putting them to the diameters of the bright Yellow ones in a subduplicate proportion of the intervals of the fits of the rays of those colours, when equally inclined to the refracting or reflecting surface, which caused those fits; that is, by putting the diameters of the rings made by the rays in the extremities and limits of the seven colours, Red, Orange, Yellow, Green, Blue, Indigo, Violet, proportional to the cube-

roots of the numbers $1, \frac{8}{9}, \frac{5}{6}, \frac{3}{4}, \frac{2}{3}, \frac{9}{16}, \frac{1}{2}$, which express the lengths of a monochord founding the notes in an eighth: for by this means the diameters of the rings of these colours will be found pretty nearly in the same proportion to one another, which they ought to have by the fifth of these Observations.

And thus I satisfied myself that these rings were of the same kind and original with those of thin plates; and by consequence that the fits, or alternate dispositions of the rays to be reflected and transmitted, are propagated to great distances from every reflecting and refracting surface. But yet to put the matter out of doubt, I added the following Observation.

Obs. 9. If these rings thus depend on the Thickness of the plate of glass, their diameters at equal distances from several speculums, made of such concavo-convex plates of glass as are ground on the same sphere, ought to be reciprocally in a subduplicate proportion of the Thicknesses of the plates of glass. And if this proportion be found true by experience, it will amount to a demonstration, that these rings, like those formed in thin plates, do depend on the Thickness of the glass. I procured therefore another concavo-convex plate of glass, ground on both sides to the same sphere with the former plate. Its thickness was $\frac{5}{12}$ parts of an inch; and the diameters of the three first bright rings, measured between the brightest parts of their orbits at the distance of six feet from the glass, were $3.4\frac{7}{8}. 5\frac{1}{8}$ inches. Now the thickness of the other glass, being $\frac{1}{4}$ of an inch, was to the thickness of this glass as $\frac{1}{4}$ to $\frac{5}{12}$; that is, as 31 to 10, or 310000000 to 100000000; and the roots of these numbers are 17607 and 10000; and in the proportion of the first of these roots to the second, are the diameters of the bright rings made in this Observation by the thinner glass, $3.4\frac{1}{8}. 5\frac{1}{8}$, to the diameters of the same rings made in the third of these Observations by the thicker glass, $1\frac{11}{16}. 2\frac{3}{8}. 2\frac{11}{12}$; that is, the diameters of the rings are reciprocally in a subduplicate proportion of the Thicknesses of the plates of glass.

So then in plates of glass which are alike concave on one side, and alike convex on the other side, and alike quick-silvered on the convex sides, and differ in nothing but their Thickness; the diameters

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diameters of the rings are reciprocally in a subduplicate proportion of the Thicknesses of the plates. And this shews sufficiently, that the rings depend on both the surfaces of the glasses. They depend on the Convex surface, because they are more luminous when that surface is quick-silvered over, than when it is without quick-silver. They depend also upon the Concave surface, because without that surface a speculum makes them not. They depend on both surfaces, and on the distances between them, because their bigness is varied by varying only that distance. And this dependence is of the same kind with that, which the colours of thin plates have on the distance of the surfaces of those plates, because the bigness of the rings, and their proportion to one another, and the variation of their bigness arising from the variation of the Thickness of the glass, and the Orders of their colours, is such as ought to result from the Propositions in the end of the third Part of this Book, derived from the phenomena of the colours of thin plates set down in the first Part.

There are yet other phenomena of these rings of colours, but such as follow from the same Propositions, and therefore confirm both the truth of those Propositions, and the analogy between these rings and the rings of colours made by very thin plates. I shall subjoin some of them.

Obs. 10. When the beam of the sun's light was reflected back from the speculum not directly to the hole in the window, but to a place a little distant from it, the common center of that spot, and of all the rings of colours fell in the middle way between the beam of the Incident light, and the beam of the Reflected light; and by consequence in the center of the spherical concavity of the speculum; whenever the chart, on which the rings of colours fell, was placed at that center. And as the beam of Reflected light, by inclining the speculum, receded more and more from the beam of Incident light, and from the common center of the coloured rings between them; those rings grew bigger and bigger, and so also did the White round spot; and new rings of colours emerged successively out of their common center, and the white spot became a white ring encompassing them; and the Incident and Reflected beams of light always fell upon the opposite

site parts of this white ring, illuminating its perimeter like two of thick transparent Plates. mock suns in the opposite parts of an iris. So then the diameter of this ring, measured from the middle of its light on one side to the middle of its light on the other side, was always equal to the distance between the middle of the Incident beam of light, and the middle of the Reflected beam, measured at the chart on which the rings appeared: and the rays, which formed this ring, were reflected by the speculum in angles equal to their angles of incidence, and by consequence to their angles of refraction at their entrance into the glass; but yet their angles of reflexion were not in the same planes with their angles of incidence.

Obs. 11. The colours of the new rings were in a contrary order to those of the former, and arose after this manner. The white round spot of light in the middle of the rings continued white to the center, till the distance of the incident and reflected beams at the chart was about $\frac{7}{8}$ parts of an inch, and then it began to grow dark in the middle. And when that distance was about $1\frac{3}{16}$ of an inch, the white spot was become a ring, encompassing a dark round spot, which in the middle inclined to Violet and Indigo. And the luminous rings encompassing it were grown equal to those dark ones, which in the four first Observations encompassed them; that is to say, the white spot was grown a white ring equal to the first of those dark rings; and the first of those luminous rings was now grown equal to the second of those dark ones, and the second of those luminous ones to the third of those dark ones, and so on. For the diameters of the luminous rings were now $1\frac{3}{16}$, $2\frac{1}{16}$, $2\frac{2}{3}$, $3\frac{1}{16}$, &c. inches.

When the distance between the incident and reflected beams of light became a little bigger, there emerged out of the middle of the dark spot, after the Indigo, a Blue; and then out of that Blue, a pale Green; and soon after a Yellow and Red. And when the colour at the center was brightest, being between Yellow and Red, the bright rings were grown equal to those rings which in the four first Observations next encompassed them; that is to say, the white spot in the middle of those rings was now become a white ring equal to the first of those bright rings; and the first of those bright ones was now come equal to the second of those, and

Reflexion and Colours and so on. For the diameters of the white ring, and of the other luminous rings encompassing it, were now $1\frac{1}{16}$, $2\frac{3}{8}$, $2\frac{11}{16}$, $3\frac{3}{8}$, &c. or thereabouts.

When the distance of the two beams of light at the chart was a little more increased, there emerged out of the middle in order after the Red, a Purple, a Blue, a Green, a Yellow, and a Red inclining much to Purple; and when the colour was brightest, being between Yellow and Red, the former Indigo, Blue, Green, Yellow and Red, were become an iris, or ring of colours, equal to the first of those luminous rings which appeared in the four first Observations; and the White ring, which was now become the second of the luminous rings, was grown equal to the second of those; and the first of those, which was now become the third ring, was become equal to the third of those; and so on. For their diameters were $1\frac{1}{16}$, $2\frac{3}{8}$, $2\frac{11}{16}$, $3\frac{3}{8}$ inches, the distance of the two beams of light, and the diameter of the white ring being $2\frac{3}{8}$ inches.

When these two beams became more distant, there emerged out of the middle of the purplish Red, first a darker round spot; and then, out of the middle of that spot, a brighter. And now the former colours (Purple, Blue, Green, Yellow, and purplish Red) were become a ring equal to the first of the bright rings mentioned in the four first Observations; and the rings about this ring were grown equal to the rings about that respectively; the distance between the two beams of light and the diameter of the white ring, which was now become the third ring, being about 3 inches.

The colours of the rings in the middle began now to grow very dilute, and if the distance between the two beams was increased half an inch or an inch more, they vanished; whilst the White ring, with one or two of the rings next it on either side, continued still visible. But if the distance of the two beams of light was still more increased, these also vanished: for the light which, coming from several parts of the hole in the window, fell upon the speculum in several angles of incidence, made rings of several bignesses, which diluted and blotted out one another, as I knew by intercepting some part of that light. For if I inter-

cepted

cepted that part which was nearest to the axis of the speculum, of thick transparent Plates. the rings would be less; if the other part, which was remotest from it, they would be bigger.

Obf. 12. When the colours of the prism were cast successively on the speculum, that ring, which in the two last Observations was White, was of the same bigness in all the colours; but the rings without it were greater in the Green than in the Blue, and still greater in the Yellow, and greatest in the Red. And, on the contrary, the rings within that White circle were less in the Green than in the Blue, and still less in the Yellow, and least in the Red. For the angles of reflexion of those rays which made this ring, being equal to their angles of incidence, the fits of every reflected ray, within the glass after reflexion, are equal in length and number to their fits of the same ray, within the glass before its incidence on the reflecting surface. And therefore since all the rays of all sorts, at their entrance into the glass, were in a fit of Transmissiion, they were also in a fit of Transmissiion at their returning to the same surface after reflexion; and by consequence were transmitted, and went out to the White ring on the chart. This is the reason why that ring was of the same bigness in all the colours, and why in a mixture of all it appears White. But in rays which are reflected in other angles, the interval of the fits of the least refrangible, being greatest, make the rings of their colour, in their progress from this White ring, either outwards or inwards, increase or decrease by the greatest steps; so that the rings of this colour without are greatest, and within least. And this is the reason why, in the last Observation, when the speculum was illuminated with White light, the Exterior rings, made by all colours, appeared Red without and Blue within; and the interior, Blue without and Red within.

These are the phenomena of thick convexo-concave plates of glass, which are every where of the same thickness. There are yet other phenomena, when these plates are a little thicker on one side than on the other; and others when the plates are more or less concave than convex, or plano-convex, or double-convex. For in all these cases the plates make rings of colours, but after various manners; all which, so far as I have yet observed, follow

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low from the Propositions in the end of the third Part of this Book, and so conspire to confirm the truth of those Propositions. But the phænomena are too various, and the calculations, whereby they follow from those Propositions, too intricate to be here prosecuted. I content myself with having prosecuted this kind of phænomena so far as to discover their cause, and by discovering it, to ratify the Propositions in the third Part of this Book.

Obs. 13. As light reflected by a lens, quick-silvered on the backside, makes the rings of colours above described, so it ought to make the like rings of colours in passing through a drop of water. At the first reflexion of the rays within the drop, some colours ought to be transmitted, as in the case of a lens, and others to be reflected back to the eye. For instance, if the diameter of a small drop or globule of water be about the 500th part of an inch, so that a Red-making ray, in passing through the middle of this globule, has 250 fits of easy Transmiffion within the globule, and that all the Red-making rays, which are at a certain distance from this middle ray round about it, have 249 fits within the globule, and all the like rays, at a certain farther distance round about it, have 248 fits, and all those at a certain farther distance 247 fits, and so on; these concentrick circles of rays, after their transmiffion, falling on a white paper, will make concentrick rings of Red upon the paper, supposing the light which passes through one single globule, strong enough to be sensible. And, in like manner, the rays of other colours will make rings of other colours. Suppose now that in a fair day the sun shines through a thin cloud of such globules of water or hail, and that the globules are all of the same bigness; and the sun, seen through this cloud, shall appear encompassed with the like concentrick rings of colours; and the diameter of the first ring of Red shall be $7\frac{1}{4}$ degrees; that of the second, $10\frac{1}{4}$ degrees; that of the third, 12 degrees 33 minutes. And accordingly as the globules of water are bigger or less, the rings shall be less or bigger. This is the Theory, and experience answers it. For in *June* 1692 I saw by reflexion, in a vessel of stagnating water, three halos, crowns, or rings of colours about the sun, like three little rain-bows, concentrick to his body. The colours

colours of the First or innermost crown were Blue next the Sun, Red without, and White in the middle between the Blue and Red. Those of the Second crown were Purple and Blue within, and pale Red without, and Green in the middle. And those of the Third were pale Blue within, and pale Red without. These crowns enclosed one another immediately, so that their colours proceeded in this continual order from the sun outward: Blue, White, Red; Purple, Blue, Green; pale Yellow and Red; pale Blue, pale Red. The diameter of the Second crown, measured from the middle of the Yellow and Red on one side of the Sun to the middle of the same colour on the other side, was $9\frac{1}{3}$ degrees, or thereabouts. The diameters of the First and Third I had not time to measure; but that of the First seemed to be about five or six degrees, and that of the Third about twelve. The like crowns appear sometimes about the moon: for in the beginning of the year 1664, *Feb.* 19th at night, I saw two such crowns about her. The diameter of the First, or innermost, was about three degrees, and that of the Second about five degrees and an half. Next about the moon was a circle of White, and next about that the inner crown; which was of a Blueish Green within next the White, and of a Yellow and Red without: and next about these colours were Blue and Green on the inside of the outward crown, and Red on the outside of it. At the same time there appeared a halo about 22 degrees 35' distant from the center of the moon. It was Elliptical, and its long diameter was perpendicular to the horizon, verging below farthest from the moon. I am told that the moon has sometimes three or more concentrick crowns of colours, encompassing one another, next about her body. The more equal the globules of water or ice are to one another, the more crowns of colours will appear, and the colours will be the more lively. The halo, at the distance of $22\frac{1}{2}$ degrees from the moon, is of another sort. By its being Oval and remoter from the moon below than above, I conclude, that it was made by refraction, in some sort of hail or snow floating in the air in an horizontal posture; the Refracting angle being about 58 or 60 degrees.

THE
THIRD BOOK
OF
OPTICKS.

Observations concerning the Inflexions of the Rays of Light, and the Colours made thereby.

Inflexion
of the

GRIMALDO has informed us, that if a beam of the sun's light be let into a dark room through a very small hole, the shadows of things, in this light, will be larger than they ought to be, if the rays went on by the bodies in strait lines; and that these shadows have three parallel fringes, bands or ranks, of coloured light adjacent to them. But if the hole be enlarged, the fringes grow broad, and run into one another, so that they cannot be distinguished. These broad shadows and fringes have been reckoned by some to proceed from the ordinary refraction of the air, but without due examination of the matter. For the circumstances of the phenomenon, so far as I have observed them, are as follows.

Obs. 1. I made in a piece of lead a small hole with a pin, whose breadth was the 42d part of an inch : for 21 of those pins laid together took up the breadth of half an inch. Through this hole,

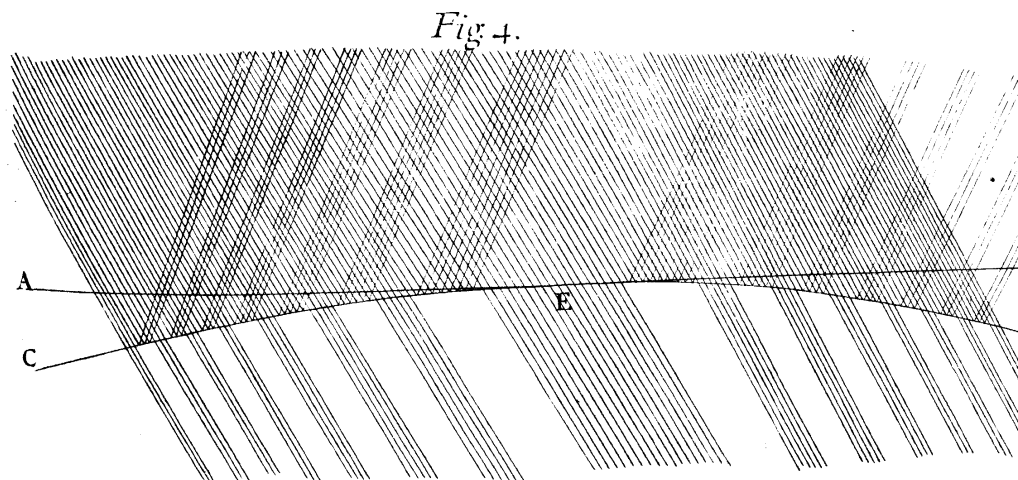
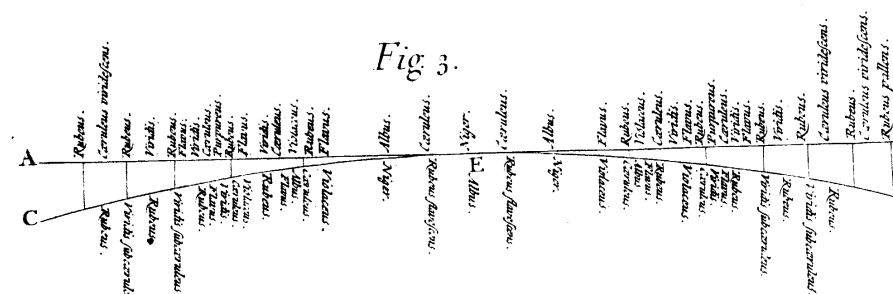
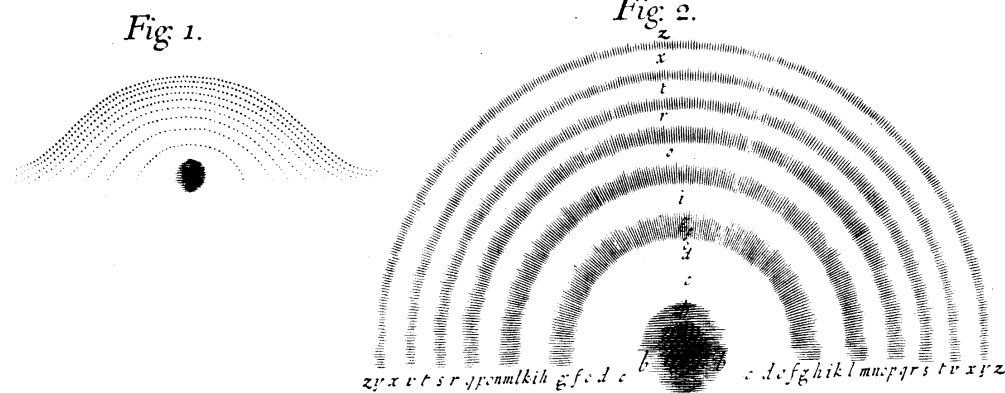


Fig 5.

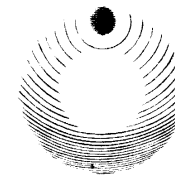


Fig 6.

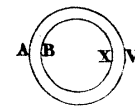
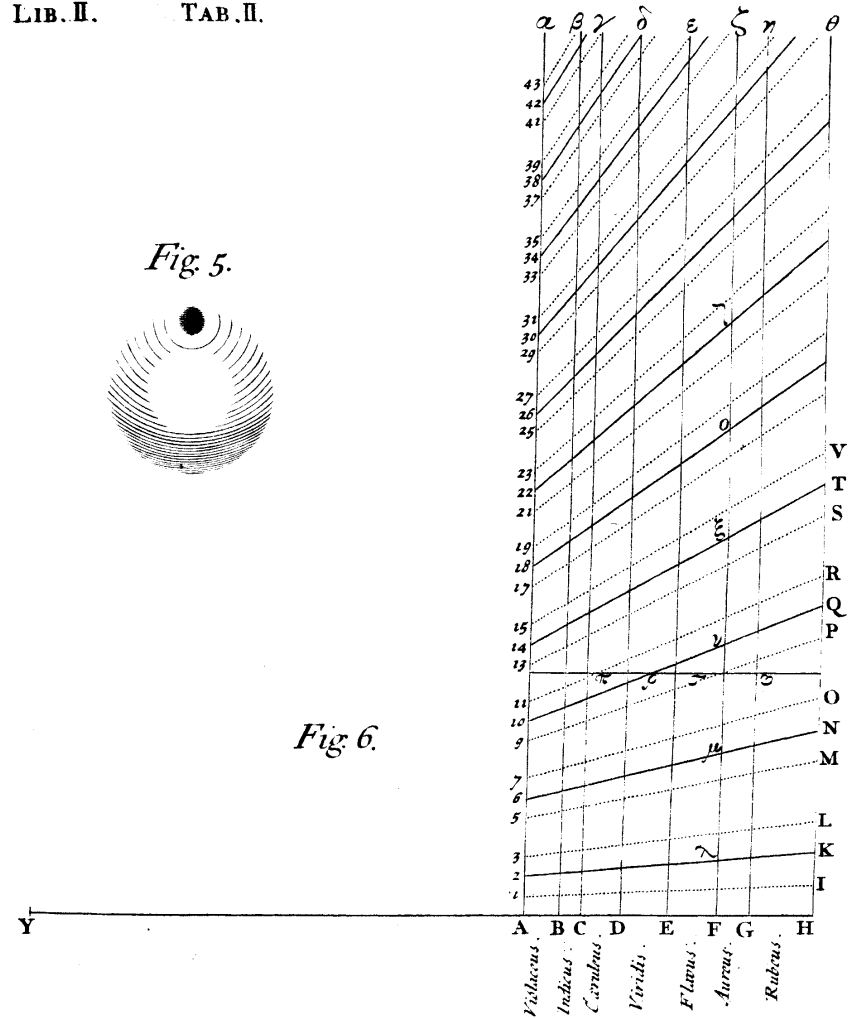
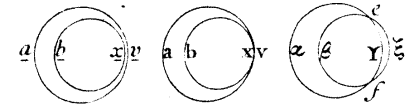


Fig. 7.



hole, I let into my darkened chamber a beam of the sun's light; ^{Rays of Light.} and found that the shadows of hairs, threads, pins, straws, and such like slender substances, placed in this beam of light, were considerably broader than they ought to be, if the rays of light passed on by these bodies in right lines. And particularly a hair of a man's head, whose breadth was but the 280th part of an inch, being held in this light, at the distance of about twelve feet from the hole, did cast a shadow, which, at the distance of four inches from the hair, was the sixtieth part of an inch broad; that is, above four times broader than the hair: and at the distance of two feet from the hair, was about the eight and twentieth part of an inch broad; that is, ten times broader than the hair: and at the distance of ten feet, was an eighth part of an inch broad; that is, 35-times broader.

Nor is it material whether the hair be encompassed with air, or with any other pellucid substance. For I wetted a polished plate of glass, and laid the hair in the water upon the glass; and then laying another polished plate of glass upon it, so that the water might fill up the space between the glasses, I held them in the aforesaid beam of light, so that the light might pass through them perpendicularly, and the shadow of the hair was, at the same distances, as big as before. The shadows of scratches, made in polished plates of glass, were also much broader than they ought to be; and the veins in polished plates of glass, did also cast the like broad shadows. And therefore the great breadth of these shadows proceeds from some other cause, than the refraction of the air.

Let the circle x [in fig. 1.] represent the middle of the hair; ADG, BEH, CFI, three rays, passing by one side of the hair at several distances; KNQ, LOR, MPS, three other rays, passing by the other side of the hair at the like distances; D, E, F, and N, O, P, the places where the rays are bent in their passage by the hair; G, H, I and Q, R, S, the places where the rays fall on a paper GQ; is the breadth of the shadow of the hair cast on the paper; and TI, vs, two rays passing to the points I and S without bending, when the hair is taken away. And it is manifest that all the light between these two rays, TI and vs, is bent in

passing by the hair, and turned aside from the shadow is; because if any part of this light were not bent, it would fall on the paper within the shadow, and there illuminate the paper, contrary to experience. And because when the paper is at a great distance from the hair, the shadow is broad, and therefore the rays, TI and VS , are at a great distance from one another, it follows that the hair acts upon the rays of light at a good distance in their passing by it. But the action is strongest on the rays, which pass by at least distances; and grows weaker and weaker, accordingly as the rays pass by at distances greater and greater, as is represented in the scheme: for thence it comes to pass, that the shadow of the hair is much broader, in proportion to the distance of the paper from the hair, when the paper is nearer the hair, than when it is at a great distance from it.

Obs. 2. The shadows of all bodies (metals, stones, glass, wood, horn, ice, &c.) in this light were bordered with three parallel fringes or bands of coloured light; whereof that which was contiguous to the shadow was broadest and most luminous; and that which was remotest from it was narrowest; and so faint, as not easily to be visible. It was difficult to distinguish the colours, unless when the light fell very obliquely upon a smooth paper, or some other smooth white body; so as to make them appear much broader, than they would otherwise do. And then the colours were plainly visible in this order: the First or innermost fringe was Violet and deep Blue next the shadow; and then light Blue, Green and Yellow in the middle, and Red without. The Second fringe was almost contiguous to the first, and the Third to the second; and both were Blue within the Yellow, and Red without; but their colours were very faint, especially those of the third. The colours therefore proceeded in this order from the shadow; Violet, Indigo, pale Blue, Green, Yellow, Red; Blue, Yellow, Red; pale Blue, pale Yellow and Red. The shadows, made by scratches and bubbles in polished plates of glass, were bordered with the like fringes of coloured light. And if plates of looking-glass, sloped off near the edges with a diamond-cut, be held in the same beam of light; the light, which passes through the parallel planes of the glass, will be bordered with the like

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fringes of colours, where those planes meet with the diamond-cut; and by this means there will sometimes appear four or five fringes of colours. Let AB , CD [in *fig. 2.*] represent the parallel planes of a looking-glass, and BD the plane of the diamond-cut, making at B a very obtuse angle with the plane AB . And let all the light, between the rays ENI and FBM , pass directly through the parallel planes of the glass, and fall upon the paper between I and M ; and all the light between the rays GO and HD be refracted by the oblique plane of the diamond-cut BD , and fall upon the paper between K and L ; and the light which passes directly through the parallel planes of the glass, and falls upon the paper between I and M , will be bordered with three or more fringes at M .

So by looking on the sun through a feather or black ribband held close to the eye, several rain-bows will appear; the shadows, which the fibres or threads cast on the *Tunica Retina*, being bordered with the like fringes of colours.

Obs. 3. When the hair was twelve feet distant from this hole, and its shadow fell obliquely upon a flat white scale of inches and parts of an inch, placed half a foot beyond it, and also when the shadow fell perpendicularly upon the same scale, placed nine feet beyond it; I measured the breadth of the shadow and fringes as accurately as I could, and found them in parts of an inch as follows.

At

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At the distance of	half a foot	nine feet
The breadth of the shadow	$\frac{1}{34}$	$\frac{1}{9}$
The breadth between the middles of the brightest light of the innermost fringes on either side the shadow	$\frac{1}{38}$ or $\frac{1}{39}$	$\frac{7}{50}$
The breadth between the middles of the brightest light of the middlemost fringes on either side the shadow	$\frac{1}{23\frac{1}{2}}$	$\frac{4}{17}$
The breadth between the middles of the brightest light of the outermost fringes on either side the shadow	$\frac{1}{18}$ or $\frac{1}{18\frac{1}{2}}$	$\frac{3}{10}$
The distance between the middles of the brightest light of the first and second fringes	$\frac{1}{10}$	$\frac{1}{21}$
The distance between the middles of the brightest light of the second and third fringes	$\frac{1}{170}$	$\frac{1}{31}$
The breadth of the luminous part (Green, White, Yellow and Red) of the first fringe	$\frac{1}{170}$	$\frac{1}{34}$
The breadth of the darker space between the first and second fringes	$\frac{1}{220}$	$\frac{1}{43}$
The breadth of the luminous part of the second fringe	$\frac{1}{290}$	$\frac{1}{53}$
The breadth of the darker space between the second and third fringes	$\frac{1}{330}$	$\frac{1}{63}$

These measures I took, by letting the shadow of the hair, at half a foot distance, fall so obliquely on the scale, as to appear twelve times broader, than when it fell perpendicularly on it at the same distance, and setting down in this Table the twelfth part of the measures I then took.

Obf. 4. When the shadow and fringes were cast obliquely upon a smooth white body, and that body was removed farther and farther from the hair, the First fringe began to appear, and look brighter than the rest of the light, at the distance of less than a quarter of an inch from the hair; and the dark line, or shadow, between that and the Second fringe, began to appear at a less distance from the hair than that of the third part of an inch. The Second fringe began to appear, at a distance from the hair of less than half an inch; and the shadow between that and the third fringe, at a distance less than an inch; and the Third fringe, at a distance less than three inches. At greater distances they became much more sensible, but kept very nearly the same proportion of their breadths and intervals, which they had at their first appearing. For the distance between the middle of the First and the

the middle of the Second fringe, was to the distance between the middle of the Second and middle of the Third fringe, as three to two, or ten to seven. And the last of these two distances was equal to the breadth of the bright light or luminous part of the First fringe. And this breadth was to the breadth of the bright light of the Second fringe as seven to four; and to the dark interval of the First and Second fringe as three to two; and to the like dark interval between the Second and Third, as two to one. For the breadths of the fringes seemed to be in the progression of the numbers 1, $\sqrt{\frac{1}{3}}$, $\sqrt{\frac{1}{5}}$, and their intervals to be in the same progression with them; that is, the fringes and their intervals together to be in the continual progression of the numbers 1, $\sqrt{\frac{1}{3}}$, $\sqrt{\frac{1}{3}}$, $\sqrt{\frac{1}{4}}$, $\sqrt{\frac{1}{5}}$, or thereabouts. And these proportions held the same, very nearly, at all distances from the hair; the dark intervals of the fringes being as broad, in proportion to the breadth of the fringes, at their first appearance, as afterwards at great distances from the hair, though not so dark and distinct.

Obf. 5. The sun shining into my darkened chamber, through a hole a quarter of an inch broad; I placed at the distance of two or three feet from the hole a sheet of paste-board, which was blackened all over on both sides, and in the middle of it had a hole, about three quarters of an inch square, for the light to pass through. And behind the hole I fastened to the paste-board with pitch the blade of a sharp knife, to intercept some part of the light, which passed through the hole. The planes of the paste-board and blade of the knife were parallel to one another, and perpendicular to the rays. And when they were so placed, that none of the sun's light fell on the pasteboard, but all of it passed through the hole to the knife, and there part of it fell upon the blade of the knife, and part of it passed by its edge: I let this part of the light, which passed by, fall on a white paper two or three feet beyond the knife; and there saw two streams of faint light shoot out both ways from the beam of light into the shadow, like the tails of comets. But because the sun's direct light, by its brightness upon the paper, obscured these faint streams, so that I could scarce see them; I made a little hole in the midst of the paper, for that light to pass through, and fall on a black cloth behind

behind it; and then I saw the two streams plainly. They were like one another, and pretty nearly equal in length and breadth, and quantity of light. Their light, at that end next the sun's direct light, was pretty strong for the space of about a quarter of an inch, or half an inch; and in all its progress from that direct light decreased gradually, till it became insensible. The whole length of either of these streams, measured upon the paper at the distance of three feet from the knife, was about six or eight inches; so that it subtended an angle, at the edge of the knife, of about 10 or 12, or at most 14 degrees. Yet sometimes I thought I saw it shoot three or four degrees farther; but with a light so very faint that I could scarce perceive it, and suspected it might (in some measure at least) arise from some other cause, than the two streams did. For placing my eye in that light, beyond the end of that stream which was behind the knife, and looking towards the knife, I could see a line of light upon its edge; and that not only when my eye was in the line of the streams, but also when it was without that line, either towards the point of the knife, or towards the handle. This line of light appeared contiguous to the edge of the knife, and was narrower than the light of the innermost fringe, and narrowest when my eye was farthest from the direct light; and therefore seemed to pass between the light of that fringe and the edge of the knife; and that which passed nearest the edge, to be most bent, though not all of it.

Obs. 6 I placed another knife by this, so that their edges might be parallel, and look towards one another; and that the beam of light might fall upon both the knives, and some part of it pass between their edges. And when the distance of their edges was about the 400th part of an inch, the stream parted in the middle, and left a shadow between the two parts. This shadow was so black and dark, that all the light which passed between the knives seemed to be bent, and turned aside to the one hand or to the other. And as the knives still approached one another, the shadow grew broader, and the streams shorter at their inward ends which were next the shadow, until, upon the con-

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tact of the knives, the whole light vanished, leaving its place to ^{Rays of} the shadow. ^{Light.}

And hence I gather, that the light which is least bent, and goes to the inward ends of the streams, passes by the edges of the knives at the greatest distance; and this distance, when the shadow begins to appear between the streams, is about the 800th part of an inch. And the light, which passes by the edges of the knives at distances still less and less, is more and more bent, and goes to those parts of the streams which are farther and farther from the direct light; because when the knives approach one another till they touch, those parts of the streams vanish last, which are farthest from the direct light.

Obs. 7. In the fifth Observation the fringes did not appear; but, by reason of the breadth of the hole in the window, became so broad as to run into one another, and, by joining, to make one continued light in the beginning of the streams. But in the sixth, as the knives approached one another, a little before the shadow appeared between the two streams, the fringes began to appear on the inner ends of the streams on either side of the direct light; three on one side, made by the edge of one knife; and three on the other side, made by the edge of the other knife. They were distinctest, when the knives were placed at the greatest distance from the hole in the window; and still became more distinct, by making the hole less; inasmuch that I could sometimes see a faint lineament of a fourth fringe, beyond the three above-mentioned. And as the knives continually approached one another, the fringes grew distincter and larger, until they vanished. The outmost fringe vanished first, and the middlemost next, and the innermost last. And after they were all vanished, and the line of light which was in the middle between them was grown very broad, enlarging itself on both sides into the streams of light described in the fifth Observation; the above-mentioned shadow began to appear in the middle of this line, and divide it along the middle into two lines of light, and increased until the whole light vanished. This enlargement of the fringes was so great, that the rays, which go to the innermost fringe, seemed to

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be bent above twenty times more when this fringe was ready to vanish, than when one of the knives was taken away.

And from this and the former Observation compared, I gather, that the light of the First fringe passed by the edge of the knife, at a distance greater than the 800th part of an inch; and the light of the Second fringe passed by the edge of the knife, at a greater distance than the light of the first fringe did; and that of the Third, at a greater distance than that of the second; and that of the streams of light, described in the fifth and sixth Observations, passed by the edges of the knives at less distances than that of any of the fringes.

Obf. 8. I caused the edges of two knives to be ground truly strait; and pricking their points into a board, so that their edges might look towards one another, and, meeting near their points, contain a rectilinear angle; I fastened their handles together with pitch, to make this angle invariable. The distance of the edges of the knives from one another, at the distance of four inches from the angular point, where the edges of the knives met, was the eighth part of an inch; and therefore the angle, contained by the edges, was about 1 degree 54'. The knives, thus fixed together, I placed in a beam of the sun's light, let into my darkened chamber through a hole the 42d part of an inch wide, at the distance of 10 or 15 feet from the hole; and let the light, which passed between their edges, fall very obliquely upon a smooth white ruler at the distance of half an inch, or an inch, from the knives; and there saw the fringes, made by the two edges of the knives, run along the edges of the shadows of the knives, in lines parallel to those edges, without growing sensibly broader; till they met in angles equal to the angle contained by the edges of the knives; and where they met and joined, they ended without crossing one another. But if the ruler was held at a much greater distance from the knives, the fringes, where they were farther from the place of their meeting, were a little narrower, and became something broader and broader, as they approached nearer and nearer to one another; and after they met they crossed one another, and then became much broader than before.

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Whence I gather, that the distances, at which the fringes pass by the knives, are not increased nor altered by the approach of the knives; but the angles, in which the rays are there bent, are much increased by that approach; and that the knife, which is nearest any ray, determines which way the ray shall be bent, and the other knife increases the bent.

Obf. 9. When the rays fell very obliquely upon the ruler, at the distance of the third part of an inch from the knives; the dark line between the first and second fringe of the shadow of one knife, and the dark line between the first and second fringe of the shadow of the other knife, met with one another at the distance of the fifth part of an inch from the end of the light, which passed between the knives at the concurrence of their edges. And therefore the distance of the edges of the knives, at the meeting of these dark lines, was the 160th part of an inch. For as four inches to the eighth part of an inch, so is any length of the edges of the knives, measured from the point of their concurrence, to the distance of the edges of the knives at the end of that length; and so is the fifth part of an inch to the 160th part. So then the dark lines above-mentioned meet in the middle of the light, which passes between the knives, where they are distant the 160th part of an inch; and the one half of that light passes by the edge of one knife, at a distance not greater than the 320th part of an inch, and, falling upon the paper, makes the fringes of the shadow of that knife; and the other half passes by the edge of the other knife, at a distance not greater than the 320th part of an inch, and, falling upon the paper, makes the fringes of the shadow of the other knife. But if the paper be held at a distance from the knives greater than the third part of an inch, the dark lines above-mentioned meet at a greater distance than the fifth part of an inch from the end of the light, which passed between the knives at the concurrence of their edges; and therefore the light, which falls upon the paper where those dark lines meet, passes between the knives, where their edges are distant above the 160th part of an inch.

For at another time, when the two knives were distant eight feet and five inches from the little hole in the window, made with a

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small pin as above; the light which fell upon the paper, where the aforesaid dark lines met, passed between the knives, where the distance between their edges was as in the following Table, when the distance of the paper from the knives was also as follows.

Distances of the Paper from the Knives in Inches.	Distance between the edges of the Knives in millesimal parts of an Inch.
1 $\frac{1}{2}$.	0'012
3 $\frac{1}{2}$.	0'020
8 $\frac{1}{2}$.	0'034
32.	0'057
96.	0'081
151.	0'087

And hence I gather that the light, which makes the fringes upon the paper, is not the same light at all distances of the paper from the knives; but when the paper is held near the knives, the fringes are made by light, which passes by the edges of the knives at a less distance, and is more bent, than when the paper is held at a greater distance from the knives.

Obs. 10. When the fringes of the shadows of the knives fell perpendicularly upon a paper, at a great distance from the knives, they were in the form of hyperbolas, and their dimensions were as follows. Let CA , CB [in *fig. 3.*] represent lines drawn upon the paper parallel to the edges of the knives, and between which all the light would fall, if it passed between the edges of the knives without reflexion; DE , a right line drawn through c , making the angles ACD , BCE , equal to one another, and terminating all the light, which falls upon the paper from the point where the edges of the knives meet; eis , fkt , and glv , three hyperbolical lines, representing the *terminus* of the shadow of one of the knives, the dark line between the first and second fringes of that shadow, and the dark line between the second and third fringes of the same shadow; xip , ykq and zlr , three other hyperbolical lines, representing the *terminus* of the shadow of the other knife, the dark line between the first and second fringes of that shadow, and the dark line between the second and third fringes of the same shadow. And conceive that these three hyperbolas are like and equal to the former three; and cross them in the points

points i , k and l ; and that the shadows of the knives are terminated, and distinguished from the first luminous fringes, by the lines eis and xip , until the meeting and crossing of the fringes; and then those lines cross the fringes in the form of dark lines, terminating the first luminous fringes within side, and distinguishing them from another light, which begins to appear at i , and illuminates all the triangular space, $ipDES$, comprehended by these dark lines, and the right line DE . Of these hyperbolas one asymptote is the line DE , and the other asymptotes are parallel to the lines CA and CB . Let rv represent a line, drawn any where upon the paper, parallel to the asymptote DE ; and let this line cross the right lines AC in m , and BC in n , and the six dark hyperbolical lines in p , q , r ; s , t , v ; and by measuring the distances ps , qt , rv , and thence collecting the lengths of the ordinates np , nq , nr , or ms , mt , mv ; and doing this at several distances of the line rv from the asymptote DE ; you may find as many points of these hyperbolas as you please, and thereby know that these curve lines are hyperbolas differing little from the Conical hyperbola. And by measuring the lines ci , ck , cl , you may find other points of these curves.

For instance, when the knives were distant from the hole in the window ten feet, and the paper from the knives nine feet, and the angle contained by the edges of the knives, to which the angle ACB is equal, was subtended by a chord which was to the radius as 1 to 32, and the distance of the line rv from the asymptote DE was half an inch: I measured the lines ps , qt , rv , and found them 0'35, 0'65, 0'98 inches respectively; and by adding to their halves the line $\frac{1}{2}mn$ (which here was the 128th part of an inch, or 0'0078 inches) the sums, np , nq , nr , were 0'1828, 0'3328, 0'4978 inches. I measured also the distances of the brightest parts of the fringes, which run between pq and st , qr and tv , and next beyond r and v , and found them 0'5, 0'8, and 1'17 inches.

Obs. 11. The sun shining into my darkened room, through a small round hole made in a plate of lead with a slender pin as above; I placed at the hole a prism to refract the light, and formed on the opposite wall the spectrum of colours, described in the third

third Experiment of the first Book. And then I found that the shadows of all bodies, held in the coloured light between the prism and the wall, were bordered with fringes of the colour of that light, in which they were held. In the full Red light they were totally Red, without any sensible Blue or Violet; and in the deep Blue light they were totally Blue, without any sensible Red or Yellow; and so in the Green light they were totally Green, excepting a little Yellow and Blue, which were mixed in the Green light of the prism. And comparing the fringes made in the several coloured lights, I found that those made in the Red light were largest, those made in the Violet were least, and those made in the Green were of a middle bigness. For the fringes with which the shadow of a man's hair were bordered, being measured cross the shadow, at the distance of six inches from the hair; the distance, between the middle and most luminous part of the First or innermost fringe on one side of the shadow, and that of the like fringe on the other side of the shadow, was, in the full Red light, $\frac{1}{37\frac{1}{2}}$ of an inch; and, in the full Violet, $\frac{1}{46}$. And the like distance, between the middle and most luminous parts of the Second fringes on either side the shadow, was, in the full Red light, $\frac{1}{22}$; and, in the Violet, $\frac{1}{27}$ of an inch. And these distances of the fringes held the same proportion, at all distances from the hair, without any sensible variation.

So then the rays, which made these fringes in the Red light, passed by the hair at a greater distance, than those did which made the like fringes in the Violet; and therefore the hair, in causing these fringes, acted alike upon the Red light, or least refrangible rays, at a greater distance, and upon the Violet, or most refrangible rays, at a less distance; and by those actions disposed the Red light into larger fringes, and the Violet into smaller, and the lights of intermediate colours into fringes of intermediate bignesses, without changing the colour of any sort of light.

When therefore the hair, in the first and second of these Observations, was held in the white beam of the Sun's light, and cast a shadow, which was bordered with three fringes of coloured light; those colours arose not from any new Modifications impressed upon the rays of light by the hair; but only from the va-

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rious Inflexions, whereby the several sorts of rays were separated from one another, which before separation, by the mixture of all their colours, composed the white beam of the sun's light; but whenever separated compose lights of the several colours, which they are originally disposed to exhibit. In this 11th Observation, where the colours are separated before the light passes by the hair, the least refrangible rays, which when separated from the rest make Red, were inflected at a greater distance from the hair; so as to make three Red fringes at a greater distance from the middle of the shadow of the hair: and the most refrangible rays, which when separated make Violet, were inflected at a less distance from the hair; so as to make three Violet fringes, at a less distance from the middle of the shadow of the hair: and other rays, of intermediate degrees of refrangibility, were inflected at intermediate distances from the hair; so as to make fringes of intermediate colours, at intermediate distances from the middle of the shadow of the hair. And in the second Observation, where all the colours are mixed in the white light which passes by the hair, these colours are separated by the various Inflexions of the rays; and the fringes, which they make, appear all together; and the Innermost fringes, being contiguous, make one broad fringe composed of all the colours in due order; the Violet lying on the inside of the fringe next the shadow; the Red, on the outside farthest from the shadow; and the Blue, Green and Yellow, in the middle. And, in like manner, the middlemost fringes of all the colours lying in order, and being contiguous, make another broad fringe composed of all the colours; and the outmost fringes of all the colours lying in order, and being contiguous, make a third broad fringe composed of all the colours. These are the three fringes of coloured light, with which the shadows of all bodies are bordered in the second Observation.

When I made the foregoing Observations, I designed to repeat most of them with more care and exactness; and to make some new ones, for determining the manner how the rays of light are bent in their passage by bodies, for making the fringes of colours with the dark lines between them. But I was then interrupted, and cannot now think of taking these things into further consideration.

deration. And since I have not finished this part of my design, I shall conclude with proposing only some Queries, in order to a farther search to be made by others.

QUERIES.

Query 1. Do not bodies act upon light at a distance; and by their action bend its rays; and is not this action (*cæteris paribus*) strongest at the least distance?

Qu. 2. Do not the rays which differ in refrangibility differ also in flexibility; and are they not by their different inflexions separated from one another, so as after separation to make the colours in the three fringes above described? And after what manner are they inflected to make those fringes?

Qu. 3. Are not the rays of light, in passing by the edges and sides of bodies, bent several times backwards and forwards, with a motion like that of an eel? And do not the three fringes of coloured light above-mentioned, arise from three such bendings?

Qu. 4. Do not the rays of light, which fall upon bodies and are reflected or refracted, begin to bend before they arrive at the bodies; and are they not Reflected, Refracted and Inflected by one and the same principle, acting variously in various circumstances?

Qu. 5. Do not bodies and light act mutually upon one another; that is to say, bodies upon light in emitting, reflecting, refracting and inflecting it; and light upon bodies, for heating them, and putting their parts into a vibrating motion, wherein heat consists?

Qu. 6. Do not Black bodies conceive heat more easily from light than those of other colours do, by reason that the light falling on them is not reflected outwards; but enters the bodies, and is often reflected and refracted within them, until it be stifled and lost?

Qu. 7. Is not the strength and vigour of the action between light and Sulphureous bodies observed above, one reason why Sulphureous bodies take fire more readily, and burn more vehemently, than other bodies do?

Qu.

(*) — *their Parts*] Nothing of what follows of this eighth Query was in the first Edition. From the words *And do not all bodies*, to the words *Oil of anniseeds*, was added (with some small variety noted below) in the first Latin edition of Dr. Clarke. And the remaining part first ap-

peared

Qu. 8. Do not all Fixed bodies, when heated beyond a certain degree, emit light and shine; and is not this emission performed by the vibrating motions of their Parts^(a). And do not all bodies, which abound with Terrestrial parts, and especially with Sulphureous ones, emit light, as often as those Parts are sufficiently agitated; whether that agitation be made by heat, or by friction, or percussion, or putrefaction, or by any vital motion, or any other cause? As for instance; sea water in a raging storm; quick-silver agitated *in Vacuo*; the back of a cat, or neck of a horse obliquely struck or rubbed in a dark place; wood, flesh and fish, while they putrefy; vapours, arising from putrefied waters, usually called *Ignes Fatui*; stacks of moist hay, or corn, growing hot by fermentation; glow-worms, and the eyes of some animals, by vital motions^(b); the vulgar *Phosphorus*^(c) agitated by the attrition of any body, or by the Acid particles of the air; ambar^(d), and some diamonds by striking, pressing or rubbing them; scrapings of steel, struck off with a flint; iron, hammered very nimbly till it become so hot as to kindle sulphur thrown upon it; the axle-trees of chariots taking fire by the rapid rotation of the wheels; and some liquors mixed with one another, whose particles come together with an impetus, as oil of vitriol distilled from its weight of nitre, and then mixed with twice its weight of oil of anniseeds. So also a globe of glass about 8 or 10 inches in diameter, being put into a frame where it may be swiftly turned round its axis, will in turning shine, where it rubs against the palm of one's hand applied to it: and if at the same time a piece of white paper, or a white cloth, or the end of one's finger, be held at the distance of about a quarter of an inch, or half an inch, from that part of the glass where it is most in motion, the Electric vapour, which is excited by the friction of the glass against the hand, will, by dashing against the white paper, cloth or finger, be put into such an agitation as to emit light; and make the white paper, cloth or finger, appear lucid like a glow-

peared in the second English edition.

(a) — *Glow-worm*. — *by vital motions*.] Not in Dr. Clarke's first Latin.

(b) — *Phosphorus*.] *Phosphorus Bononiensis, radiis luminis agitatus; Phosphorus vulgaris*, &c. Dr. Clarke's first Latin.

(c) *Ambar*.] Not in Dr. Clarke's first Latin.

Queries
1x, x.

worm; and in rushing out of the glass, will sometimes push against the finger so as to be felt. And the same things have been found by rubbing a long and large cylinder of glass or amber with a paper held in one's hand, and continuing the Friction till the glass grew warm.

Of the form
of Fire.

Qu. 9. Is not Fire a body heated so hot, as to emit light copiously? For what else is a red hot iron than fire? And what else is a burning coal than red hot wood?

—of Flame.

Qu. 10. Is not Flame a vapour, fume or exhalation heated red hot, that is, so hot as to shine? For bodies do not flame without emitting a copious fume, and this fume burns in the flame. The *Ignis Fatuus* is a vapour shining without heat; and is there not the same difference between this vapour and flame, as between rotted wood shining without heat and burning coals of fire? In distilling hot spirits, if the head of the still be taken off, the vapour, which ascends out of the still, will take fire at the flame of a candle, and turn into flame, and the flame will run along the vapour from the candle to the still. Some bodies heated by motion or fermentation, if the heat grow intense, fume copiously; and if the heat be great enough, the fumes will shine, and become flame. Metals in fusion do not flame for want of a copious fume, except spelter, which fumes copiously, and thereby flames. All flaming bodies, as oil, tallow, wax, wood, fossil coals, pitch, sulphur, by flaming waste and vanish into burning smoke; which smoke, if the flame be put out, is very thick and visible, and sometimes smells strongly, but in the flame loses its smell by burning; and, according to the nature of the smoke, the flame is of several colours; as that of sulphur, blue; that of copper opened with sublimate, green; that of tallow, yellow; that of camphire, white^(c). Smoke passing through flame cannot but grow red hot; and red hot smoke can have no other appearance than that of flame^(d). When gun-powder takes fire, it goes away into flaming smoke. For the charcoal and sulphur easily take fire,

(c) — that of Camphire white.] Not in the first edition.

(d) — flame.] What follows of this 10th Query was not in the first edition, but appeared in the first Latin of Dr. Clarke.

(e) — which arise from them.] What follows of this 11th Query was not in the first edition, but

fire, and set fire to the nitre; and the spirit of the nitre being thereby rarified into vapour, rushes out with explosion much after the manner that the vapour of water rushes out of an æoli-pile; the sulphur also, being volatile, is converted into vapour, and augments the explosion. And the Acid vapour of the sulphur (namely, that which distills under a bell into oil of sulphur) entering violently into the Fixed body of the nitre, sets loose the spirit of the nitre, and excites a greater fermentation; whereby the heat is farther augmented, and the fixed body of the nitre is also rarified into fume, and the explosion is thereby made more vehement and quick. For if salt of Tartar be mixed with gun-powder, and that mixture be warmed till it takes fire; the explosion will be more violent and quick than that of gun-powder alone: which cannot proceed from any other cause than the action of the vapour of the gun-powder upon the salt of tartar, whereby that salt is rarified. The explosion of gun-powder arises therefore from the violent action, whereby all the mixture, being quickly and vehemently heated, is rarified and converted into fume and vapour: which vapour, by the violence of that action, becoming so hot as to shine, appears in the form of flame.

Qu. 11. Do not Great bodies conserve their heat the longest, their parts heating one another; and may not Great dense and Fixed bodies, when heated beyond a certain degree, emit light so copiously, as by the emission and re-action of its light, and the reflexions and refractions of its rays within its pores, to grow still hotter, till it comes to a certain period of heat, such as is that of the sun? And are not the sun and fixed stars great earths vehemently hot; whose heat is conserved by the greatness of the bodies, and the mutual action and re-action between them, and the light which they emit; and whose parts are kept from fuming away, not only by their Fixity, but also by the vast weight and density of the atmospheres incumbent upon them, and very strongly compressing them, and condensing the vapours and exhalations which arise from them^(e)? For if water be made

Formal cause
of the Sun.

but appeared in the first Latin of Dr. Clarke, except that we find nothing there answering to the final words, and a very small quantity of vapours and exhalations, which were first inserted in the second English.

Queries
xi, xii.

warm in any pellucid vessel emptied of air, that water in the *vacuum* will bubble and boil as vehemently, as it would in the open air in a vessel set upon the fire till it conceives a much greater heat. For the weight of the incumbent atmosphere keeps down the vapours, and hinders the water from boiling, until it grow much hotter than is requisite to make it boil *in Vacuo*. Also a mixture of tin and lead, being put upon a red hot iron *in Vacuo*, emits a fume and flame; but the same mixture in the open air, by reason of the incumbent atmosphere, does not so much as emit any fume, which can be perceived by sight. In like manner the great weight of the atmosphere, which lies upon the globe of the sun, may hinder bodies there from rising up and going away from the sun, in the form of vapours and fumes; unless by means of a far greater heat, than that which, on the surface of the earth, would very easily turn them into vapours and fumes. And the same great weight may condense these vapours and exhalations, as soon as they shall at any time begin to ascend from the sun, and make them presently fall back again into him; and by that action increase his heat, much after the manner that in our earth the air increases the heat of a culinary fire. And the same weight may hinder the globe of the sun from being diminished, unless by the emission of light, and a very small quantity of vapours and exhalations.

Of the sense
of Sight.

Qu. 12. Do not the rays of light, in falling upon the bottom of the eye, excite vibrations in the *Tunica Retina*? Which vibrations, being propagated along the solid fibres of the Optick nerves into the brain, cause the sense of seeing. For because dense bodies conserve their heat a long time, and the densest bodies conserve their heat the longest; the vibrations of their parts are of a lasting nature, and therefore may be propagated along solid fibres of uniform dense matter to a great distance, for conveying into the brain the impressions made upon all the organs of sense. For that motion, which can continue long in one and the same part of a body, can be propagated a long way from one part to another, supposing the body homogeneous, so that the motion may not be reflected, refracted, interrupted or disordered by any unevenness of the body.

Qu.

Qu. 13. Do not several sorts of rays make vibrations of several bignesses; which according to their bignesses excite sensations of several colours: much after the manner that the vibrations of the air, according to their several bignesses, excite sensations of several Sounds? And particularly do not the most refrangible rays excite the shortest vibrations, for making a sensation of deep Violet; the least refrangible, the largest, for making a sensation of deep Red; and the several intermediate sorts of rays, vibrations of several intermediate bignesses, to make sensations of the several intermediate colours?

Queries xiii, xiv, xv.
Of the perception of different colours.

Qu. 14. May not the harmony and discord of colours, arise from the proportions of the vibrations propagated through the fibres of the Optick nerves into the brain; as the harmony and discord of sounds arise from the proportions of the vibrations of the air? For some colours, if they be viewed together, are agreeable to one another, as those of gold and Indigo, and others disagree.

Harmony of colours.

Qu. 15. Are not the species of objects, seen with both eyes, united where the Optick nerves meet before they come into the brain; the fibres on the right side of both nerves uniting there, and after union going thence into the brain, in the nerve which is on the right side of the head; and the fibres on the left side of both nerves uniting in the same place, and after union going into the brain, in the nerve which is on the left side of the head; and these two nerves meeting in the brain, in such a manner that their fibres make but one entire species or picture; half of which, on the right side of the sensorium, comes from the right side of both eyes, through the right side of both Optick nerves, to the place where the nerves meet, and from thence on the right side of the head into the brain; and the other half, on the left side of the sensorium, comes in like manner from the left side of both eyes. For the Optick nerves of such animals, as look the same way with both eyes, as of men, dogs, sheep, oxen, &c. meet before they come into the brain; but the Optick nerves of such animals as do not look the same way with both eyes, as of fishes and of the chameleon, do not meet, if I am rightly informed.

Reason of single Vision.

Queries
xvi, xvii.
Duration of
the vibrations
of the Optic
Nerve, which
produce the
sense of
Sight.

Qu. 16. When a man in the dark presses either corner of his eye with his finger, and turns his eye away from his finger, he will see a circle of colours like those in the feather of a peacock's tail. If the eye and the finger remain quiet, these colours vanish in a second minute of time; but if the finger be moved with a quavering motion, they appear again^(a). Do not these colours arise from such motions, excited in the bottom of the eye, by the pressure and motion of the finger, as at other times are excited there by light for causing vision? And do not the motions once excited continue about a second of time before they cease⁽ⁱ⁾? And when a man by a stroke upon his eyes sees a flash of light, are not the like motions excited in the *Retina* by the stroke^(k)? And when a coal of fire, moved nimbly in the circumference of a circle, makes the whole circumference appear like a circle of fire; is it not because the motions excited in the bottom of the eye, by the rays of light, are of a lasting nature; and continue till the coal of fire, in going round, returns to its former place? And considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature?

Efficient
cause of the
fits of Trans-
mission and
Reflexion.

Qu. 17. If a stone be thrown into stagnating water, the waves excited thereby continue some time to arise, in the place where the stone fell into the water, and are propagated from thence in concentrick circles, upon the surface of the water, to great distances. And the vibrations or tremors excited in the air by percussion, continue a little time to move from the place of percussion, in concentrick spheres, to great distances. And in like manner, when a ray of light falls upon the surface of any pellucid body, and is there refracted or reflected: may not waves of vibrations, or tremors, be thereby excited in the refracting or reflecting medium at the point of incidence; and continue to arise there, and to be propagated from thence, as long as they continue to arise and be propagated, when they are excited in the bottom of the eye by the pressure or motion of the finger, or by the light which comes from the coal of fire in experiments above-mentioned?

^(a) If the eye — appear again.] Not in the first edition, nor in Dr. Clarke's first Latin.

⁽ⁱ⁾ And do not — before they cease.] Not in the first edition, nor in Dr. Clarke's first Latin.

^(k) — Retina by the stroke.] With these words the Book ends in the first edition. The remain-
ing

mentioned? And are not these vibrations propagated from the point of incidence to great distances? And do they not overtake the rays of light, and by overtaking them successively, do they not put them into the fits of easy Reflexion and easy Transmission described above? For if the rays endeavour to recede from the densest part of the vibration, they may be alternately accelerated and retarded by the vibrations overtaking them.

Qu. 18. If in two large tall cylindrical vessels of glass inverted, two little thermometers be suspended so as not to touch the vessels, and the air be drawn out of one of these vessels, and these vessels, thus prepared, be carried out of a cold place into a warm one; the thermometer *in Vacuo* will grow warm as much, and almost as soon as the thermometer which is not *in Vacuo*: and when the vessel is carried back into the cold place, the thermometer *in Vacuo* will grow cold, almost as soon as the other thermometer. Is not the heat of the warm room conveyed through the *Vacuum* by the vibrations of a much subtler medium than air, which, after the air was drawn out, remained in the *Vacuum*? And is not this medium the same with that medium by which light is refracted or reflected, and by whose vibrations light communicates heat to bodies, and is put into fits of easy Reflexion and easy Transmission? And do not the vibrations of this medium, in hot bodies, contribute to the intenseness and duration of their heat? And do not hot bodies communicate their heat to contiguous cold ones, by the vibrations of this medium propagated from them into the cold ones? And is not this medium exceedingly more rare and subtle than the air, and exceedingly more elastick and active? And doth it not readily pervade all bodies? And is it not (by its elastick force) expanded through all the heavens?

Qu. 19. Doth not the refraction of light proceed from the different density of this æthereal medium in different places, the light receding always from the denser parts of the medium? And is not the density thereof greater in free and open spaces,

ing paragraph of this Query, and the eight Queries that immediately follow, namely, the 17th, 18th, 19th, 20th, 21st, 22d, 23d, 24th, first appeared in the second English edition, and are not found in Dr. Clarke's first Latin.

Queries
25, 26.

void of air and other grosser bodies, than within the pores of water, glass, crystal, gems, and other compact bodies? For when light passes through glass or crystal, and, falling very obliquely upon the farther surface thereof, is totally reflected; the total reflexion ought to proceed rather from the Density and Vigour of the medium without and beyond the glass, than from the Rarity and Weakness thereof.

This medium
denser in
empty spaces
than within
dense bodies.

Qu. 20. Doth not this æthereal medium, in passing out of water, glass, crystal, and other compact and dense bodies into empty spaces, grow denser and denser by degrees; and by that means refract the rays of light not in a point, but by bending them gradually in curve lines? And doth not the gradual condensation of this medium extend to some distance from the bodies; and thereby cause the inflexions of the rays of light, which pass by the edges of dense bodies, at some distance from the bodies?

The mechanical
efficient
of Gravity.

Qu. 21. Is not this medium much rarer within the dense bodies of the sun, stars, planets and comets, than in the empty celestial spaces between them? And in passing from them to great distances, doth it not grow denser and denser perpetually; and thereby cause the Gravity of those great bodies towards one another, and of their parts towards the bodies; every body endeavouring to go from the denser parts of the medium towards the rarer? For if this medium be rarer within the sun's body than at its surface, and rarer there than at the hundredth part of an inch from its body, and rarer there than at the fiftieth part of an inch from its body, and rarer there than at the orb of *Saturn*; I see no reason why the increase of density should stop any where, and not rather be continued through all distances from the sun to *Saturn*, and beyond. And though this increase of density may at great distances be exceeding slow, yet if the Elastick force of this medium be exceeding great, it may suffice to impel bodies from the denser parts of the medium towards the rarer, with all that power which we call Gravity. And that the Elastick force of this medium is exceeding great, may be gathered from the swiftness of its vibrations. Sounds move about 1140 *English* feet in a second minute of time; and in seven or eight minutes of time, they move about one hundred *English* miles. Light moves from

from the sun to us in about seven or eight minutes of time; ^{Queries} which distance is about 70000000 *English* miles, supposing the ^{xxi, xxii.} horizontal parallax of the sun to be about 12". And the vibrations or pulses of this medium, that they may cause the alternate fits of easy Transmissiion and easy Reflexion, must be swifter than light; and by consequence above 700000 times swifter than sounds. And therefore the Elastick force of this medium, in proportion to its density, must be above 700000 × 700000 (that is, above 490000000000) times greater than the Elastick force of the air is in proportion to its density. For the velocities of the pulses of elastick mediums are in a subduplicate *ratio* of the elasticities and the rarities of the mediums taken together.

As attraction is stronger in small magnets than in great ones, in proportion to their bulk; and gravity is greater in the surfaces of small planets than in those of great ones, in proportion to their bulk; and small bodies are agitated much more by Electric Attraction than great ones; so the smallness of the rays of light may contribute very much to the power of the agent, by which they are refracted. And so if any one should suppose that *æther*, like our air, may contain particles which endeavour to recede from one another (for I do not know what this *æther* is) and that its particles are exceedingly smaller than those of air, or even than those of light: the exceeding smallness of its particles may contribute to the greatness of the force, by which those particles may recede from one another; and thereby make that medium exceedingly more rare and Elastick than air, and by consequence exceedingly less able to resist the motions of projectiles, and exceedingly more able to press upon gross bodies, by endeavouring to expand itself.

Qu. 22. May not planets and comets, and all gross bodies, perform their motions more freely, and with less resistance in this ^{Small resistance of the æthereal medium.} æthereal medium than in any fluid, which fills all space adequately without leaving any pores, and by consequence is much denser than quick-silver or gold? And may not its resistance be so small as to be inconsiderable? For instance; if this *æther* (for so I will call it) should be supposed 700000 times more Elastick than our air, and above 700000 times more rare; its resistance

Queries

xxiii, xxiv.

would be above 600000000 times less than that of water. And so small a resistance would scarce make any sensible alteration in the motions of the planets in ten thousand years. If any one would ask how a medium can be so rare, let him tell me how the air, in the upper parts of the atmosphere, can be above an hundred thousand times rarer than gold. Let him also tell me, how an Electric body can by friction emit an exhalation so rare and subtle, and yet so potent, as by its emission to cause no sensible diminution of the weight of the electric body; and to be expanded through a sphere, whose diameter is above two feet, and yet to be able to agitate and carry up leaf-copper, or leaf-gold, at the distance of above a foot from the electric body? And how the *effluvia* of a magnet can be so rare and subtle, as to pass through a plate of glass without any resistance or diminution of their force, and yet so potent as to turn a magnetick needle beyond the glass?

The æthereal
medium the
mechanical
efficient of
Vision.

Qu. 23. Is not Vision performed chiefly by the vibrations of this medium, excited in the bottom of the eye by the rays of light, and propagated through the solid, pellucid and uniform *Capillamenta* of the Optick nerves into the place of sensation? And is not Hearing performed by the vibrations either of this or some other medium, excited in the Auditory nerves by the tremors of the air, and propagated through the solid, pellucid and uniform *Capillamenta* of those nerves into the place of sensation? And so of the other senses.

—of Animal
motion.

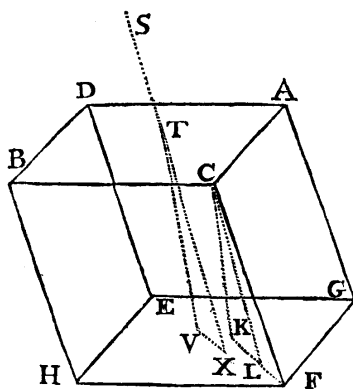
Qu. 24. Is not Animal motion performed by the vibrations of this medium, excited in the brain by the power of the will, and propagated from thence through the solid, pellucid and uniform *Capillamenta* of the nerves into the muscles, for contracting and dilating them? I suppose that the *Capillamenta* of the nerves are each of them Solid and Uniform, that the vibrating motion of the æthereal medium may be propagated along them from one end to the other uniformly, and without interruption: for obstructions in the nerves create palsies. And that they may be sufficiently uniform, I suppose them to be pellucid when viewed singly; though the reflexions in their cylindrical surfaces may make the whole nerve, composed of many *Capillamenta*, appear Opaque and White.

White. For Opacity arises from Reflecting surfaces, such as may Query xxv. disturb and interrupt the motions of this medium.

Qu. 25. Are there not other original properties of the rays of light, besides those already described? An instance of another original property we have in the refraction of Iceland Crystal, described first by *Erasmus Bartholine*, and afterwards more exactly by *Hugenius*, in his Book *De la Lumiere*. This crystal is a pellucid fissile stone, clear as water or crystal of the rock, and without colour; enduring a red heat without losing its transparency, and, in a very strong heat, calcining without fusion: steeped a day or two in water, it loses its natural polish: being rubbed on cloth, it attracts pieces of straws and other light things, like amber or glass; and with *aqua fortis* it makes an ebullition. It seems to be a sort of Talk, and is found in form of an oblique parallelopiped, with six parallelogram sides and eight solid angles. The obtuse angles of the parallelograms are each of them 101 degrees and 52 minutes; the acute ones 78 degrees and 8 minutes. Two of the solid angles opposite to one another, as c and E, are encompassed each of them with three of these Obtuse angles; and each of the other six, with one Obtuse and two Acute ones. It cleaves easily in planes parallel to any of its sides, and not in any other planes. It cleaves with a glossy polite surface, not perfectly plane, but with some little unevenness. It is easily scratched, and, by reason of its softness, it takes a polish very difficultly. It polishes better upon polished looking-glass than upon metal; and perhaps better upon pitch, leather or parchment. Afterwards it must be rubbed with a little oil or white of an egg, to fill up its scratches; whereby it will become very transparent and polite. But for several experiments, it is not necessary to polish it. If a piece of this crystalline stone be laid upon a book; every letter of the book, seen through it, will appear double, by means of a double refraction. And if any beam of light falls either perpendicularly, or in any oblique angle, upon any surface of this crystal; it becomes divided into two beams by means of the same double refraction. Which beams are of the same colour with the incident beam of light, and seem equal to one another in the Quantity of their light,

Query xxv.
Island Crystal.

light, or very nearly equal. One of these refractions is performed by the usual rule of Opticks; the sine of incidence out of air into this crystal being to the sine of refraction as five to three. The other refraction, which may be called the unusual refraction, is performed by the following Rule.



Let ADBC represent the Refracting surface of the crystal; c, the biggest solid angle at that surface; GEHF, the opposite surface; and CK, a perpendicular on that surface. This perpendicular makes with the edge of the crystal CF, an angle of 19 deg. 3'. Join KF; and in it take KL, so that the angle KCL be 6 deg. 40'. and the angle LCF 12 deg. 23'. And if ST represent any beam of light incident at T, in any angle, upon

the Refracting surface ADBC, let TV be the refracted beam determined by the given proportion of the sines 5 to 3, according to the usual rule of Opticks. Draw VX parallel and equal to KL. Draw it the same way from V, in which L lieth from K; and joining TX, this line TX shall be the other refracted beam, carried from T to X, by the unusual refraction.

If therefore the incident beam ST be perpendicular to the Refracting surface, the two beams TV and TX, into which it shall become divided, shall be parallel to the lines CK and CL; one of those beams going through the crystal perpendicularly, as it ought to do by the usual laws of Opticks; and the other, TX, by an unusual refraction, diverging from the perpendicular, and making with it an angle VTX of about $6\frac{2}{3}$ degrees, as is found by experience. And hence the plane VTX, and such like planes, which are parallel to the plane CFK, may be called the planes of Perpendicular refraction. And the coast, towards which the lines KL and VX are drawn, may be called the coast of an unusual refraction.

In

In like manner crystal of the rock has a double refraction: but the differences of the two refractions is not so great and manifest as in island crystal. Queries xxv, xxvi.

When the beam ST, incident on island crystal, is divided into two beams, TV and TX, and these two beams arrive at the farther surface of the glass; the beam TV, which was refracted at the first surface after the usual manner, shall be again refracted entirely after the usual manner at the second surface; and the beam TX, which was refracted after the unusual manner in the first surface, shall be again refracted entirely after the unusual manner in the second surface; so that both these beams shall emerge out of the second surface in lines parallel to the first incident beam ST.

And if two pieces of island crystal be placed one after another, in such manner that all the surfaces of the latter be parallel to all the corresponding surfaces of the former: the rays, which are refracted after the usual manner in the first surface of the first crystal, shall be refracted after the usual manner in all the following surfaces; and the rays, which are refracted after the unusual manner in the first surface, shall be refracted after the unusual manner in all the following surfaces. And the same thing happens, though the surfaces of the crystals be any ways inclined to one another, provided that their planes of Perpendicular refraction be parallel to one another.

And therefore there is an original difference in the rays of light, by means of which some rays are in this experiment constantly refracted after the usual manner, and others constantly after the unusual manner: for if the difference be not original, but arises from new modifications impressed on the rays at their first refraction, it would be altered by new modifications in the three following refractions; whereas it suffers no alteration, but is constant, and has the same effect upon the rays in all the refractions. The unusual refraction is therefore performed by an Original Property of the rays. And it remains to be enquired, whether the rays have not more Original Properties than are yet discovered?

Qu. 26. Have not the rays of light several sides, endued with several original Properties? For if the planes of Perpendicular refraction of the second crystal, be at right angles with the planes of The rays of Light have several sides.

of

Query xxiv.
Ray of
Light

of Perpendicular refraction of the first crystal; the rays, which are refracted after the usual manner in passing through the first crystal, will be all of them refracted after the unusual manner in passing through the second crystal; and the rays, which are refracted after the unusual manner in passing through the first crystal, will be all of them refracted after the usual manner in passing through the second crystal. And therefore there are not two sorts of rays differing in their nature from one another, one of which is constantly and in all positions refracted after the usual manner, and the other constantly and in all positions after the unusual manner. The difference between the two sorts of rays, in the Experiment mentioned in the 25th Question, was only in the positions of the sides of the rays to the planes of Perpendicular refraction. For one and the same ray is here refracted sometimes after the usual, and sometimes after the unusual manner, according to the position which its sides have to the crystals. If the side of the ray is posited the same way to both crystals, it is refracted after the same manner in them both: but if that side of the ray, which looks towards the coast of the unusual refraction of the first crystal, be 90 degrees from that side of the same ray, which looks towards the coast of the unusual refraction of the second crystal (which may be effected by varying the position of the second crystal to the first, and by consequence to the rays of light) the ray shall be refracted after several manners in the several crystals. There is nothing more required to determine whether the rays of light, which fall upon the second crystal, shall be refracted after the usual or after the unusual manner; but to turn about this crystal, so that the coast of this crystal's unusual refraction may be on this or on that side of the ray. And therefore every ray may be considered as having four sides or quarters; two of which, opposite to one another, incline the ray to be refracted after the unusual manner, as often as either of them are turned towards the coast of unusual refraction; and the other two, whenever either of them are turned towards the coast of unusual refraction, do not incline it to be otherwise refracted than after the usual manner. The two first may therefore be called the sides of unusual refraction. And since these dispositions were

in the rays, before their incidence on the second, third and fourth surfaces of the two crystals; and suffered no alteration, so far as appears, by the refraction of the rays in their passage through those surfaces; and the rays were refracted by the same laws in all the four surfaces: it appears, that those dispositions were in the rays originally, and suffered no alteration by the first refraction; and that by means of those dispositions the rays were refracted at their incidence on the first surface of the first crystal; some of them after the usual, and some of them after the unusual manner, accordingly as their sides of unusual refraction were then turned towards the coast of the unusual refraction of that crystal, or sideways from it.

Every ray of light has therefore two opposite sides, originally endued with a property on which the unusual refraction depends, and with other two opposite sides not endued with that property. And it remains to be enquired, whether there are not more properties of light by which the sides of the rays differ, and are distinguished from one another.

In explaining the difference of the sides of the rays above-mentioned, I have supposed that the rays fall perpendicularly on the first crystal. But if they fall obliquely on it, the success is the same. Those rays which are refracted after the usual manner in the first crystal, will be refracted after the unusual manner in the second crystal, supposing the planes of Perpendicular refraction to be at right angles with one another, as above; and on the contrary.

If the planes of the Perpendicular refraction of the two crystals be neither parallel nor perpendicular to one another, but contain an Acute angle: the two beams of light which emerge out of the first crystal, will be each of them divided into two more at their incidence on the second crystal. For in this case the rays, in each of the two beams, will some of them have their sides of unusual refraction, and some of them their other sides, turned toward the coast of the unusual refraction of the second crystal.

Q^y. 27. Are not all hypotheses erroneous which have hitherto been invented for explaining the phaenomena of light, by new modifications of the rays? For those phaenomena depend not upon

Query
XXVIII.

new modifications, as has been supposed, but upon the Original and Unchangeable properties of the rays.

Light consists
not in preffion,
or motion,
propagated
through a
fluid Medium.

24. 28. Are not all hypotheses erroneous, in which light is supposed to consist in preffion or motion, propagated through a fluid medium? For in all these hypotheses, the phænomena of light have been hitherto explained, by supposing that they arise from new modifications of the rays; which is an erroneous supposition.

If light consisted only in preffion propagated without actual motion; it would not be able to agitate and heat the bodies, which refract and reflect it. If it consisted in motion propagated to all distances in an instant; it would require an infinite force every moment, in every shining particle, to generate that motion. And if it consisted in preffion or motion, propagated either in an instant or in time, it would bend into the shadow. For preffion or motion cannot be propagated in a fluid in right lines beyond an obstacle, which stops part of the motion, but will bend and spread every way into the quiescent medium, which lies beyond the obstacle⁽¹⁾. Gravity tends downwards; but the pressure of water, arising from gravity, tends every way with equal force; and is propagated as readily, and with as much force, sideways as downwards, and through crooked passages as through strait ones. The waves on the surface of stagnating water, passing by the sides of a broad obstacle which stops part of them, bend afterwards, and dilate themselves gradually into the quiet water behind the obstacle. The waves, pulses or vibrations of the air, wherein sounds consist, bend manifestly; though not so much as the waves of water. For a bell, or a cannon, may be heard beyond a hill, which intercepts the sight of the sounding body; and sounds are propagated as readily through crooked pipes, as through streight ones. But light is never known to follow crooked passages, nor to bend into the shadow. For the fixed stars, by the interposition of any of the planets, cease to be seen. And so do the parts of the sun by the interposition of the moon, *Mercury* or *Venus*. The rays, which pass very near to the edges of any body, are bent a little by the action of the body, as we shewed above; but this

⁽¹⁾ Princip. Lib. II. Prop. XLII.

bending

bending is not towards, but from the shadow, and is performed ^{Query} only in the passage of the ray by the body, and at a very small ^{XXVIII.} distance from it. So soon as the ray is past the body, it goes right on.

To explain the unusual refraction of island crystal by preffion or motion propagated, has not hitherto been attempted (to my knowledge) except by *Huygens*; who for that end supposed two several vibrating mediums within that crystal. But when he tried the refractions in two successive pieces of that crystal, and found them such as is mentioned above: he confessed himself at a loss for explaining them. For preffions or motions, propagated from a shining body through an uniform medium, must be on all sides alike; whereas by those experiments it appears, that the rays of light have different properties in their different sides. He suspected that the pulses of *æther*, in passing through the first crystal, might receive certain new modifications; which might determine them to be propagated in this or that medium within

*Mais pour dire comment
cela se fait, je n'ay rien
trouvé jusqu'ici qui me sa-
tisfasse.* C. H. de la lumi-
ère. c. 5. p. 91.

the second crystal, according to the position of that crystal. But what modifications those might be, he could not say; nor think of any thing satisfactory in that point. And if he had known that the unusual refraction depends not on new modifications, but on the original and unchangeable dispositions of the rays, he would have found it as difficult to explain how those dispositions, which he supposed to be impressed on the rays by the first crystal, could be in them before their incidence on that crystal; and in general, how all rays, emitted by shining bodies, can have those dispositions in them from the beginning. To me, at least, this seems inexplicable, if light be nothing else than preffion or motion propagated through *æther*.

And it is as difficult to explain by these hypotheses, how rays can be alternately in fits of easy Reflexion and easy Transmiffion; unless perhaps one might suppose, that there are in all space two æthereal vibrating mediums, and that the vibrations of one of them constitute light, and the vibrations of the other are swifter, and as often as they overtake the vibrations of the first, put them

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into

into those fits (^m). But how two *æthers* can be diffused through all space, one of which acts upon the other, and by consequence is re-acted upon, without retarding, shattering, dispersing and confounding one another's motions, is inconceivable. And against filling the heavens with fluid mediums, unless they be exceeding rare, a great objection arises from the regular and very lasting motions of the planets and comets in all manner of courses through the heavens. For thence it is manifest, that the heavens are void of all sensible resistance, and by consequence of all sensible matter.

For the resisting power of fluid mediums arises partly from the attrition of the parts of the medium, and partly from the *vis inertiae* of the matter. That part of the resistance of a spherical body, which arises from the attrition of the parts of the medium, is very nearly as the diameter; or at the most, as the *factum* of the diameter, and the velocity of the spherical body together. And that part of the resistance, which arises from the *vis inertiae* of the matter, is as the square of that *factum* (ⁿ). And by this difference the two sorts of resistance may be distinguished from one another in any medium; and these being distinguished, it will be found that almost all the resistance of bodies of a competent magnitude, moving in air, water, quick-silver, and such like fluids, with a competent velocity, arises from the *vis inertiae* of the parts of the fluid.

Now that part of the resisting power of any medium, which arises from the tenacity, friction or attrition of the parts of the medium, may be diminished by dividing the matter into smaller parts, and making the parts more smooth and slippery: but that part of the resistance, which arises from the *vis inertiae*, is proportional to the density of the matter, and cannot be diminished by dividing the matter into smaller parts, nor by any other means than

(^m) The remaining part of this paragraph, in the first of the two *Latin* editions of Dr. Clarke, stood thus. Verum duo ibi confingere Ætherum genera, ubi nulla ratio cogat ut vel unum quidem admittamus; existimare porro duo Ætherum genera ita in omnia spatia unà inesse posse, ut tamen invicem non intermiscantur, nec in unum Medium coeant; comminisci denique duo illa Ætherum genera distinctas ita habere posse vibrationes, ut tamen duo Luminis genera non constituent; hæc quidem mihi videntur difficultates inexplicabiles. Præterea nulla esse omnino istiusmodi media fluida, inde colligo, quod Planetæ & Cometæ regulari adeo & diuturno motu per spatia

than by decreasing the density of the medium. And for these reasons the density of fluid mediums is very nearly proportional to their resistance. Liquors which differ not much in density, as water, spirit of wine, spirit of turpentine, hot oil, differ not much in resistance. Water is thirteen or fourteen times lighter than quick-silver, and by consequence thirteen or fourteen times rarer; and its resistance is less than that of quick-silver in the same proportion, or thereabouts, as I have found by experiments made with pendulums. The open air, in which we breathe, is eight or nine hundred times lighter than water, and by consequence eight or nine hundred times rarer; and accordingly its resistance is less than that of water in the same proportion, or thereabouts; as I have also found by experiments made with pendulums (^o). And in thinner air the resistance is still less; and at length, by rarifying the air, becomes insensible. For some feathers falling in the open air meet with great resistance; but in a tall glass, well emptied of air, they fall as fast as lead or glass, as I have seen tried several times. Whence the resistance seems still to decrease in proportion to the density of the fluid. For I do not find by any experiments, that bodies moving in quick-silver, water or air, meet with any other sensible resistance, than what arises from the density and tenacity of those sensible fluids; as they would do, if the pores of those fluids, and all other spaces, were filled with a dense and subtile fluid (P). Now if the resistance in a vessel, well emptied of water, was but an hundred times less than in the open air, it would be about a million of times less than in quick-silver. But it seems to be much less in such a vessel; and still much less in the heavens, at the height of three or four hundred miles from the earth, or above. For Mr. Boyle has shewed, that air may be rarified above ten thousand times in vessels of glass; and the heavens are much emptier of air, than any vacuum we can make below. For since the air is compressed by the weight of the incumbent atmosphere, and the density of air is

spatia cælestia undique & quaquaversum & in omnes partes ferantur. Inde enim liquet spatia cælestia omnis sensibilis resistentiæ, & consequenter omnis sensibilis materiæ, expertia esse.

In the second *English* edition, and all that succeeded, it appeared as we now give it.

(^o) Princip. Lib. II. Prop. xxxv. Cor. 5. and Prop. xxxviii. Cor. 1.

(^p) Princip. Lib. II. Sect. vi. Scholium.

(^p) See Princip. Lib. II. Sect. vi. Scholium towards the end.

Query
xxviii.
No dense
matter

proportional to the force compressing it; it follows by computation, that at the height of about seven and a half *English* miles from the earth, the air is four times rarer than at the surface of the earth; and at the height of 15 miles, it is sixteen times rarer than that at the surface of the earth; and at the height of $22\frac{1}{2}$, 30, or 38 miles, it is respectively 64, 256, or 1024 times rarer, or thereabouts; and at the height of 76, 152, 228 miles, it is about 1000000, 1000000000000 or 10000000000000000 times rarer; and so on (9).

Heat promotes fluidity very much, by diminishing the tenacity of bodies. It makes many bodies fluid, which are not fluid in cold; and increases the fluidity of tenacious liquors, as of oil, balsam and honey, and thereby decreases their resistance. But it decreases not the resistance of water considerably; as it would do, if any considerable part of the resistance of water arose from the attrition, or tenacity, of its parts. And therefore the resistance of water arises principally, and almost entirely, from the *vis inertiae* of its matter; and by consequence, if the heavens were as dense as water, they would not have much less resistance than water; if as dense as quick-silver, they would not have much less resistance than quick-silver; if absolutely dense, or full of matter without any *vacuum*, let the matter be never so subtle and fluid, they would have a greater resistance than quick-silver. A solid globe in such a medium would lose above half its motion in moving three times (1) the length of its diameter; and a globe not solid, such as are the planets, would be retarded sooner. And therefore to make way for the regular and lasting motions of the planets and comets, it is necessary to empty the heavens of all matter; except perhaps some very thin vapours, steams or *effluvia*, arising from the atmospheres of the earth, planets and comets, and from such an exceeding rare æthereal medium as we described above. A dense fluid can be of no use for explaining

(9) Concerning the rarity of the air at all heights above the earth's surface, see the sixth Section of my discourse on the Barometer published in the Volume of the Philosophical Transactions.

(1) — *three times*.] Our Author seems to make the supposition of a solid, or absolutely dense globe moving in a fluid *Plenum*. The density therefore of the globe is supposed equal to that of the fluid, wherein it is supposed to move. Should we not, therefore, according to the 4th Cor. of Princip. Lib. II. Prop. xxviii. for *three times*, read *twice*?

the

the phænomena of Nature, the motions of the planets and comets being better explained without it (s). It serves only to disturb and retard the motions of those great bodies, and make the frame of Nature languish: and in the pores of bodies, it serves only to stop the vibrating motions of their parts, wherein their heat and activity consists. And as it is of no use, and hinders the operations of Nature, and makes her languish; so there is no evidence for its existence, and therefore it ought to be rejected. And if it be rejected, the hypotheses that light consists in pressure or motion propagated through such a medium, are rejected with it.

And for rejecting such a medium, we have the authority of those the oldest and most celebrated philosophers of *Greece* and *Phœnicia*, who made a *vacuum* and atoms, and the gravity of atoms, the first principles of their philosophy; tacitly attributing gravity to some other cause than dense matter (t). Later philosophers banish the consideration of such a cause out of Natural Philosophy, feigning hypotheses for explaining all things mechanically, and referring other causes to metaphysics. Whereas the main business of natural philosophy is to argue from phænomena without feigning hypotheses, and to deduce causes from effects, till we come to the very First cause; which certainly is not mechanical: and not only to unfold the mechanism of the world, but chiefly to resolve these and such like Questions. *What is there in places [almost] (u) empty of matter, and whence is it that the sun and planets gravitate towards one another, without [dense] (v) matter between them? Whence is it that Nature doth nothing in vain; and whence arises all that order and beauty which we see in the world? To what end are comets; and whence is it that planets move all one and the same way in orbs concentrick, while comets move all manner of ways in orbs very excentrick; and what*

(s) *A dense fluid—without it*.] For this sentence we had in Dr. Clarke's first *Latin*, *Materia illa fictitia & commentitia, quæ cæli sunt repleti, nullo modo utilis est ad explicanda Phænomena Naturæ; quando Planetarum quidem & Cometarum motus, ope Gravitatis melius explicantur sine illâ; gravitasque per illam nondum fuit explicata.*

(t) — *other cause than dense matter*.] *Alii alicui causæ à materiâ diversæ.* Dr. Clarke's first *Latin*.

(v) — *almost—dense*.] These two words have nothing answering to them in Dr. Clarke's first *Latin*. *binders*.

Query
xxviii,
xxix.

binders the fixed stars from falling upon one another (x)? How came the bodies of animals to be contrived with so much art, and for what ends were their several Parts? Was the eye contrived without skill in Opticks, and the ear without knowledge of sounds? How do the motions of the body follow from the will; and whence is the instinct in animals? Is not the sensory of animals that place, to which the sensitive substance is present; and into which the sensible species of things are carried through the nerves and brain, that there they may be perceived by their immediate presence to that substance? And these things being rightly dispatched, Does it not appear from phenomena, that there is a Being incorporeal, living, intelligent, omnipresent, who, in infinite space, as it were in his sensory (y), sees the things themselves intimately, and thoroughly perceives them, and comprehends them wholly by their immediate presence to himself: of which things the images only, carried through the organs of sense into our little sensoriums, are there seen and beheld by that which in us perceives and thinks. And though every true step made in this philosophy brings us not immediately to the knowledge of the First cause, yet it brings us nearer to it, and on that account is to be highly valued.

Of the Form
of Light.

Qu. 29. Are not the rays of light very small bodies emitted from shining substances? For such bodies will pass through uniform mediums in right lines, without bending into the shadow; which is the nature of the rays of light. They will also be capable of several properties, and be able to conserve their properties unchanged in passing through several mediums; which is another condition of the rays of light. Pellucid substances act upon the rays of light, at a distance, in refracting, reflecting and inflecting them; and the rays mutually agitate the parts of those substances at a distance, for heating them; and this action and reaction, at a distance, very much resembles an Attractive force between

(x) — *the fixed Stars from falling.* Quo minus Sol & Stellæ fixæ in se mutuò irruant. Dr. Clarke's first Latin.

(y) — *in infinite space as it were in his sensory.*

Νε; δὲ οἱ ἀψυδοὶ, βασιλικοὶ, ἀπὸ τοῦ αἵματος,
ὡς δὲ πάλαι κλονεῖται καὶ φρεσὶται ὑπὸ τῆς ἡσυχίας
Αἰσθητῆρος, ὑπὸ αὐτοῦ κλονεῖται, ὑπὸ μὲν οὖσα,
ἢ ἄλλῃ Διὸς καὶ ὑπερβαλὸς Κρονίου.

tween bodies. If refraction be performed by attraction of the rays, the sines of incidence must be to the sines of refraction in a given proportion; as we shewed in our principles of philosophy (z): and this rule is true by experience. The rays of light, in going out of glass into a vacuum, are bent towards the glass; and if they fall too obliquely on the vacuum, they are bent backwards into the glass, and totally reflected; and this reflexion cannot be ascribed to the resistance of an absolute vacuum, but must be caused by the power of the glass attracting the rays at their going out of it into the vacuum, and bringing them back. For if the farther surface of the glass be moistened with water or clear oil, or liquid and clear honey; the rays, which would otherwise be reflected, will go into the water, oil, or honey; and therefore are not reflected, before they arrive at the farther surface of the glass, and begin to go out of it. If they go out of it into the water, oil or honey; they go on, because the attraction of the glass is almost balanced, and rendered ineffectual by the contrary attraction of the liquor. But if they go out of it into a vacuum, which has no attraction to balance that of the glass; the attraction of the glass either bends and refracts them, or brings them back and reflects them. And this is still more evident by laying together two prisms of glass, or two object-glasses of very long telescopes, the one plane, the other a little convex, and so compressing them that they do not fully touch, nor are too far asunder. For the light, which falls upon the farther surface of the first glass, where the interval between the glasses is not above the ten hundred thousandth part of an inch, will go through that surface, and through the air or vacuum between the glasses, and enter into the second glass; as was explained in the first, fourth and eighth Observations of the first Part of the second Book. But if the second glass be taken away; the light, which goes out of the second surface of the first glass into the air or vacuum, will not go on forwards, but turns back into the

I am not singular in imagining a resemblance between Sir Isaac Newton's notion of space as the sensory of God, and that which we find in these Orphic Verses. The learned Gesner has remarked it. *His versibus poterat uti Clarchius ad naturam spatii sui declarandam, quatenus illud sensorium quoddam. Deici posse putavit.* It must be confessed, this opinion is more tolerable in poetry than in philosophy.

(z) Princip. Lib. II. Prop. xciv.

Query xxxix. first glass, and is reflected; and therefore it is drawn back by the power of the first glass, there being nothing else to turn it back. Nothing more is requisite for producing all the variety of colours and degrees of refrangibility, than that the rays of light be bodies of different sizes; the least of which may make Violet, the weakest and darkest of the colours, and be more easily diverted by Refracting surfaces from the right course; and the rest, as they are bigger and bigger, may make the stronger and more lucid colours, Blue, Green, Yellow and Red, and be more and more difficultly diverted. Nothing more is requisite for putting the rays of light into fits of easy Reflexion and easy Transmiffion, than that they be small bodies, which by their attractive powers, or some other force, stir up vibrations in what they act upon; which vibrations being swifter than the rays, overtake them successively, and agitate them; so as by turns to increase and decrease their velocities, and thereby put them into those fits. And lastly, the unusual refraction of island crystal looks very much as if it were performed by some kind of Attractive virtue, lodged in certain sides both of the rays, and of the particles of the crystal. For were it not for some kind of disposition, or virtue, lodged in some sides of the particles of the crystal, and not in their other sides, and which inclines and bends the rays towards the coast of unusual refraction; the rays, which fall perpendicularly on the crystal, would not be refracted towards that coast rather than towards any other coast, both at their incidence and at their emergence, so as to emerge perpendicularly, by a contrary situation of the coast of unusual refraction, at the second surface; the crystal acting upon the rays, after they have passed through it and are emerging into the air, or, if you please, into a *vacuum*. And since the crystal by this disposition or virtue does not act upon the rays, unless when one of their sides of unusual refraction looks towards that coast; this argues a virtue or disposition in those sides of the rays, which answers to and sympathizes with that virtue or disposition of the crystal, as the poles of two magnets answer to one another. And as magnetism may be intended and remitted, and is found only in the magnet and in iron: so this virtue, of refracting the Perpendicular, rays is greater in island crystal, less

less in crystal of the rock, and is not yet found in other bodies. I do not say that this virtue is Magnetical; it seems to be of another kind: I only say, that whatever it be, it is difficult to conceive how the rays of light, unless they be bodies, can have a permanent virtue in two of their sides, which is not in their other sides; and this without any regard to their position to the space, or medium, through which they pass. Queries
xix, xx.

What I mean in this Question by a *vacuum*, and by the attractions of the rays of light towards glass or crystal, may be understood by what was said in the 18th, 19th and 20th Questions (^{2a}).

24. 30. Are not gross bodies and light convertible into one another; and may not bodies receive much of their activity from the particles of light which enter their composition? For all Fixed bodies, being heated, emit light so long as they continue sufficiently hot; and light mutually stops in bodies, as often as its rays strike upon their parts, as we shewed above. I know no body less apt to shine than water; and yet water, by frequent distillations, changes into fixed earth, as Mr. Boyle has tried; and then this earth, being enabled to endure a sufficient heat, shines by heat like other bodies. Mutual convertibility of
gross bodies
and light.

The changing of bodies into light, and light into bodies, is very conformable to the course of Nature, which seems delighted with transmutations. Water, which is a very fluid tasteless salt, she changes by heat into vapour, which is a sort of air; and by cold into ice, which is a hard, pellucid, brittle, fusible stone; and this stone returns into water by heat, and vapour returns into water by cold. Earth by heat becomes fire, and by cold returns into earth. Dense bodies by fermentation rarify into several sorts of air; and this air by fermentation, and sometimes without it, returns into dense bodies. Mercury appears sometimes in the form of a fluid metal; sometimes in the form of a hard brittle metal; sometimes in the form of a corrosive pellucid salt, called sublimate; sometimes in the form of a tasteless, pellucid, volatile white earth, called *Mercurius dulcis*; or in that of a red opaque volatile earth, called Cinnabar; or in that of

(*) This last paragraph of this Query was not in Dr. Clark's first Latin.

mixing, does not this heat argue a great motion in the parts of the liquors? And does not this motion argue, that the parts of the two liquors, in mixing, coalesce with violence; and by consequence, rush towards one another with an accelerated motion? And when *aqua fortis*, or spirit of vitriol, poured upon filings of iron, dissolves the filings with a great heat and ebullition, is not this heat and ebullition effected by a violent motion of the parts? And does not that motion argue, that the acid parts of the liquor rush towards the parts of the metal with violence, and run forcibly into its pores, till they get between its outmost particles and the main mass of the metal, and surrounding those particles loosen them from the main mass, and set them at liberty to float off into the water? And when the Acid particles, which alone would distill with an easy heat, will not separate from the particles of the metal without a very violent heat, doth not this confirm the attraction between them?

When spirit of vitriol, poured upon common salt, or salt-petre, makes an ebullition with the salt and unites with it; and in distillation the spirit of the common salt or salt-petre comes over much easier than it would do before; and the acid part of the spirit of vitriol stays behind; does not this argue, that the fixed alcali of the salt attracts the acid spirit of the vitriol more strongly than its own spirit; and not being able to hold them both, lets go its own? And when oil of vitriol is drawn off from its weight of nitre, and from both the ingredients a compound spirit of nitre is distilled, and two parts of this spirit are poured on one part of oil of cloves or caraway seeds, or of any ponderous oil of Vegetable or Animal substances, or oil of turpentine thickened with a little balsam of sulphur, and the liquors grow so very hot in mixing, as presently to send up a burning flame: does not this very great and sudden heat argue, that the two liquors mix with violence; and that their parts, in mixing, run towards one another with an accelerated motion, and clash with the greatest force? And is it not for the same reason that well rectified spirit of wine, poured on the same compound spirit, flashes; and that the *Pulvis fulminans*, composed of sulphur, nitre, and salt of tartar, goes off with a more sudden and violent explosion than

gun-powder; the Acid spirits of the sulphur and nitre rushing ^{Attractions.} towards one another, and towards the salt of tartar, with so great a violence, as by the shock to turn the whole at once into vapour and flame? Where the dissolution is slow, it makes a slow ebullition and a gentle heat; and where it is quicker, it makes a greater ebullition with more heat; and where it is done at once, the ebullition is contracted into a sudden blast or violent explosion, with a heat equal to that of fire and flame. So when a drachm of the above-mentioned compound spirit of nitre was poured upon half a drachm of oil of caraway seeds *in vacuo*; the mixture immediately made a flash like gun-powder, and burst the exhausted receiver, which was a glass six inches wide, and eight inches deep. And even the gross body of sulphur powdered, and with an equal weight of iron filings, and a little water made into paste, acts upon the iron; and in five or six hours grows too hot to be touched, and emits a flame. And by these experiments, compared with the great quantity of sulphur with which the earth abounds, and the warmth of the interior parts of the earth, and hot springs, and burning mountains, and with damps, mineral corruscations, earthquakes, hot suffocating exhalations, hurricanes and spouts; we may learn, that sulphureous steams abound in the bowels of the earth and ferment with minerals, and sometimes take fire with a sudden corruscation and explosion; and if pent up in subterraneous caverns, burst the caverns with a great shaking of the earth, as in springing of a mine. And then the vapour, generated by the explosion, expiring through the pores of the earth, feels hot and suffocates; and makes tempests and hurricanes; and sometimes causes the land to slide, or the sea to boil; and carries up the water thereof in drops, which by their weight fall down again in spouts. Also some sulphureous steams, at all times when the earth is dry, ascending into the air, ferment there with Nitrous acids; and, sometimes taking fire, cause lightning and thunder, and fiery meteors. For the air abounds with acid vapours fit to promote fermentations; as appears by the rusting of iron and copper in it, the kindling of fire by blowing, and the beating of the heart by means of respiration. Now the above-mentioned motions are so great

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great and violent, as to shew that in fermentations, the particles of bodies which almost rest, are put into new motions by a very potent principle, which acts upon them only when they approach one another, and causes them to meet and clash with great violence, and grow hot with the motion, and dash one another into pieces, and vanish into air, and vapour, and flame.

When salt of tartar *per deliquium*, being poured into the solution of any metal, precipitates the metal, and makes it fall down to the bottom of the liquor in the form of mud: does not this argue, that the Acid particles are attracted more strongly by the salt of tartar than by the metal, and by the stronger attraction go from the metal to the salt of tartar? And so when a solution of iron in *Aqua fortis* dissolves the *Lapis Calaminaris*, and lets go the iron; or a solution of copper dissolves iron immersed in it, and lets go the copper; or a solution of silver dissolves copper, and lets go the silver; or a solution of mercury in *Aqua fortis*, being poured upon iron, copper, tin or lead, dissolves the metal and lets go the mercury: does not this argue, that the Acid particles of the *Aqua fortis* are attracted more strongly by the *Lapis Calaminaris* than by iron; and more strongly by iron, than by copper; and more strongly by copper, than by silver; and more strongly by iron, copper, tin and lead, than by mercury? And is it not for the same reason that iron requires more *Aqua fortis* to dissolve it than copper, and copper more than the other metals; and that of all metals, iron is dissolved most easily, and is most apt to rust; and next after iron, copper?

When oil of vitriol is mixed with a little water, or is run *per deliquium*, and in distillation the water ascends difficultly, and brings over with it some part of the oil of vitriol in the form of spirit of vitriol, and this spirit being poured upon iron, copper, or salt of tartar, unites with the body and lets go the water: doth not this shew, that the Acid spirit is attracted by the water, and more attracted by the Fixed body than by the water, and therefore lets go the water to close with the fixed body? And is it not for the same reason that the water and Acid spirits, which are mixed together in vinegar, *Aqua fortis*, and spirit of salt, cohere and rise together in distillation; but if the *menstruum* be poured on

on salt of tartar, or on lead or iron, or any Fixed body which it can dissolve, the acid by a stronger attraction adheres to the body, and lets go the water? And is it not also from a mutual attraction, that the spirits of foot and sea-salt unite and compose the particles of sal-armoniac; which are less volatile than before, because grosser and freer from water; and that the particles of sal-armoniac in sublimation carry up the particles of antimony, which will not sublime alone; and that the particles of mercury, uniting with the acid particles of spirit of salt, compose mercury sublimate; and with the particles of sulphur, compose cinnabar; and that the particles of spirit of wine and spirit of urine well rectified unite; and, letting go the water which dissolved them, compose a consistent body; and that in subliming cinnabar from salt of tartar, or from quick lime, the sulphur, by a stronger attraction of the salt or lime, lets go the mercury, and stays with the fixed body; and that when mercury sublimate is sublimed from antimony, or from regulus of antimony, the spirit of salt lets go the mercury, and unites with the antimonial metal, which attracts it more strongly; and stays with it, till the heat be great enough to make them both ascend together; and then carries up the metal with it in the form of a very fusible salt, called butter of antimony; although the spirit of salt alone be almost as volatile as water, and the antimony alone as fixed as lead?

When *Aqua fortis* dissolves silver and not gold, and *Aqua regia* dissolves gold and not silver; may it not be said, that *Aqua fortis* is subtiler enough to penetrate gold as well as silver, but wants the Attractive force to give it entrance; and that *Aqua regia* is subtiler enough to penetrate silver as well as gold, but wants the Attractive force to give it entrance? For *Aqua regia* is nothing else than *Aqua fortis* mixed with some spirit of salt, or with sal-armoniac; and even common salt, dissolved in *Aqua fortis*, enables the *menstruum* to dissolve gold, though the salt be a gross body. When therefore spirit of salt precipitates silver out of *Aqua fortis*; is it not done by attracting and mixing with the *Aqua fortis*, and not attracting, or perhaps repelling silver? And when water precipitates antimony out of the sublimate of antimony and sal-armoniac, or out of butter of antimony; is it not done by its dissolving,

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diffolving, mixing with, and weakening the sal-armoniac or spirit of salt, and its not attracting, or perhaps repelling the antimony? And is it not for want of an Attractive virtue between the parts of water and oil, of quick-silver and antimony, of lead and iron, that these substances do not mix; and by a weak attraction, that quick-silver and copper mix difficultly; and from a strong one, that quick-silver and tin, antimony and iron, water and salts, mix readily? And in general, is it not from the same principle that heat congregates homogeneous bodies, and separates heterogeneous ones?

When arsenick with soap gives a regulus, and with mercury sublimate a volatile fusible salt, like butter of antimony; doth not this shew, that arsenick, which is a substance totally volatile, is compounded of fixed and volatile parts, strongly cohering by a mutual attraction, so that the volatile will not ascend without carrying up the fixed? And so, when an equal weight of spirit of wine and oil of vitriol are digested together, and in distillation yield two fragrant and volatile spirits, which will not mix with one another, and a fixed black earth remains behind; doth not this shew, that oil of vitriol is composed of volatile and fixed parts strongly united by attraction, so as to ascend together in form of a volatile, acid, fluid salt, until the spirit of wine attracts and separates the volatile parts from the fixed? And therefore, since oil of sulphur *per campanam* is of the same nature with oil of vitriol, may it not be inferred, that sulphur is also a mixture of volatile and fixed parts so strongly cohering by attraction, as to ascend together in sublimation. By dissolving flowers of sulphur in oil of turpentine, and distilling the solution, it is found that sulphur is composed of an inflammable thick oil or fat bitumen, an acid salt, a very fixed earth, and a little metal. The three first were found not much unequal to one another, the fourth in so small a quantity as scarce to be worth considering. The Acid salt, dissolved in water, is the same with oil of sulphur *per campanam*; and abounding much in the bowels of the earth, and particularly in markasites, unites itself to the other ingredients of the markasite, which are, bitumen, iron, copper and earth, and with them compounds alume, vitriol and sulphur.

With

With the earth alone it compounds alum; with the metal alone, *Attractious*, or metal and earth together, it compounds vitriol; and with the bitumen and earth it compounds sulphur. Whence it comes to pass, that markasites abound with those three minerals. And is it not from the mutual attraction of the ingredients, that they stick together for compounding these minerals, and that the bitumen carries up the other ingredients of the sulphur, which without it would not sublime? And the same question may be put concerning all, or almost all the gross bodies in nature. For all the parts of animals and vegetables are composed of substances volatile and fixed, fluid and solid, as appears by their analysis; and so are salts and minerals, so far as chemists have been hitherto able to examine their composition.

When mercury sublimate is re-sublimed with fresh mercury, and becomes *mercurius dulcis*, which is a white tasteless earth scarce dissolvable in water; and *mercurius dulcis*, re-sublimed with spirit of salt, returns into mercury sublimate; and when metals, corroded with a little Acid, turn into rust, which is an earth tasteless and indissolvable in water; and this earth, imbibed with more acid, becomes a metallic salt; and when some stones, as spar of lead, dissolved in proper *menstruums*, become salts: do not these things shew, that salts are dry earth and watery acid united by attraction; and that the earth will not become, a salt without so much acid as makes it dissolvable in water? Do not the sharp and pungent tastes of acids arise from the strong attraction, whereby the acid particles rush upon and agitate the particles of the tongue? And when metals are dissolved in acid *menstruums*, and the acids, in conjunction with the metal, act after a different manner; so that the compound has a different taste, much milder than before, and sometimes a sweet one: is it not because the acids adhere to the metallic particles, and thereby lose much of their activity? And if the acid be in too small a proportion to make the compound dissolvable in water, will it not, by adhering strongly to the metal, become unactive and lose its taste, and the compound be a tasteless earth? For such things; as are not dissolvable by the moisture of the tongue, act not upon the taste.

Elective

As gravity makes the sea flow round the denser and weightier parts of the globe of the earth; so the attraction may make the watery acid flow round the denser and compacter particles of earth, for composing the particles of salt. For otherwise the acid would not do the office of a medium between the earth and common water, for making salts dissolvable in the water; nor would salt of tartar readily draw off the acid from dissolved metals, nor metals the acid from mercury. Now as in the great globe of the earth and sea, the densest bodies by their gravity sink down in water, and always endeavour to go towards the center of the globe; so in particles of salt, the densest matter may always endeavour to approach the center of the particle: so that a particle of salt may be compared to a chaos; being dense, hard, dry, and earthy in the center; and rare, soft, moist, and watery in the circumference. And hence it seems to be, that salts are of a lasting nature, being scarce destroyed, unless by drawing away their watery parts by violence, or by letting them soak into the pores of the central earth by a gentle heat in putrefaction, until the earth be dissolved by the water, and separated into smaller particles; which, by reason of their smallness, make the rotten compound appear of a black colour. Hence also it may be, that the parts of animals and vegetables preserve their forms, and assimilate their nourishment; the soft and moist nourishment easily changing its texture by a gentle heat and motion, till it becomes like the dense, hard, dry, and durable earth in the center of each particle. But when the nourishment grows unfit to be assimilated, or the central earth grows too feeble to assimilate it, the motion ends in confusion, putrefaction and death.

If a very small quantity of any salt or vitriol be dissolved in a great quantity of water, the particles of the salt or vitriol will not sink to the bottom, though they be heavier in *specie* than the water; but will evenly diffuse themselves into all the water, so as to make it as saline at the top as at the bottom. And does not this imply, that the parts of the salt or vitriol recede from one another, and endeavour to expand themselves, and get as far asunder as the quantity of water, in which they float, will allow? And does not this endeavour imply, that they have a Repulsive

pulsive force by which they fly from one another; or, at least, Attractions. that they attract the water more strongly than they do one another? For as all things ascend in water, which are less attracted than water, by the gravitating power of the earth; so all the particles of salt which float in water, and are less attracted than water by any one particle of salt, must recede from that particle, and give way to the more attracted water.

When any saline vapour is evaporated to a cuticle and let cool, the salt concretes in regular figures; which argues, that the particles of the salt, before they concreted, floated in the liquor at equal distances in rank and file, and by consequence that they acted upon one another by some power, which at equal distances is equal, at unequal distances unequal. For by such a power they will range themselves uniformly, and without it they will float irregularly, and come together as irregularly. And since the particles of island crystal act all the same way upon the rays of light, for causing the unusual refraction; may it not be supposed, that in the formation of this crystal, the particles not only ranged themselves in rank and file for concreting in regular figures, but also, by some kind of polar virtue, turned their homogeneous sides the same way?

The parts of all homogeneous hard bodies, which fully touch one another, stick together very strongly. And for explaining how this may be, some have invented hooked atoms, which is begging the question; and others tell us, that bodies are glued together by Rest; that is, by an occult quality, or rather by nothing^(d): and others, that they stick together by conspiring motions, that is, by relative Rest amongst themselves. I had rather infer from their cohesion, that their particles attract one another by some force, which in immediate contact is exceeding strong, at small distances performs the Chemical operations above-mentioned, and reaches not far from the particles with any sensible effect.

All bodies seem to be composed of hard particles: for otherwise fluids would not congeal; as water, oils, vinegar, and spirit or oil of vitriol do by freezing; mercury, by fumes of lead; spi-

(d) — *by rest—by nothing.*—Quiete; hoc est planè nihilo. Dr. Clarke's first Latin.

Elastic

rit of nitre and mercury, by dissolving the mercury and evaporating the flegm; spirit of wine and spirit of urine, by deflegming and mixing them; and spirit of urine and spirit of salt, by subliming them together to make sal-armoniac. Even the rays of light seem to be hard bodies; for otherwise they would not retain different properties in their different sides. And therefore hardness may be reckoned the property of all uncompound matter. At least, this seems to be as evident as the universal impenetrability of matter. For all bodies, so far as experience reaches, are either hard, or may be hardened; and we have no other evidence of universal impenetrability, besides a large experience without an experimental exception. Now if compound bodies are so very hard as we find some of them to be, and yet are very porous, and consist of parts which are only laid together; the simple particles which are void of pores, and were never yet divided, must be much harder. For such hard particles being heaped up together, can scarce touch one another in more than a few points; and therefore must be separable by much less force, than is requisite to break a solid particle, whose parts touch in all the space between them, without any pores or interstices to weaken their cohesion. And how such very hard particles, which are only laid together, and touch only in a few points, can stick together, and that so firmly as they do, without the assistance of something which causes them to be attracted or pressed towards one another, is very difficult to conceive.

The same thing I infer also from the cohering of two polished marbles in *Vacuo*; and from the standing of quick-silver in the barometer at the height of 50, 60 or 70 inches, or above, whenever it is well purged of air and carefully poured in, so that its parts

(^{cc}) *The atmosphere by its weight, &c.*] Instead of the sequel of this paragraph, Dr. Clarke's first *Latin* had what follows.

Nonnulli existimant, marmora illa compressa esse Æthere quodam ambiente, argentumque vivum eodem Æthere sursum in tubum impelli. Verum si ætheri illi transitus patet, vel per argentum vivum, vel per vitrum; fieri non potest, ut is argentum vivum sursum in tubum impellat. Et si transitus ei per neutrum patet, jam non poterit is permittere, ut argentum vivum sublevet; quomodo illud sublevis quidem, si vitrum succutiat, adeoque argentum vivum à se disjungat; si vero argentum vivum habeat in se bullulas aliquas aëris, quæ impendant, quo minus partes ejus contingant, & cohaerant inter se. Atque etiam idem experimentum observatum fuit in aqua, ab omni prius aëre probè depurgatâ. Quum argentum vivum congelatum sit funis plumbi, vel aqua frigore,

parts be every where contiguous both to one another and to the glass (^{cc}). The atmosphere by its weight presses the quick-silver into the glass, to the height of 29 or 30 inches. And some other agent raises it higher, not by pressing it into the glass, but by making its parts stick to the glass, and to one another. For upon any discontinuation of parts, made either by bubbles or by shaking the glass, the whole mercury falls down to the height of 29 or 30 inches.

And of the same kind with these experiments are those that follow (^{ff}). If two plane polished plates of glass (suppose two pieces of a polished looking-glass) be laid together, so that their sides be parallel, and at a very small distance from one another, and then their lower edges be dipped into water, the water will rise up between them. And the less the distance of the glass is, the greater will be the height to which the water will rise. If the distance be about the hundredth part of an inch, the water will rise to the height of about an inch; and if the distance be greater or less in any proportion, the height will be reciprocally proportional to the distance very nearly. For the attractive force of the glasses is the same, whether the distance between them be greater or less; and the weight of the water drawn up is the same, if the height of it be reciprocally proportional to the distance of the glasses. And in like manner, water ascends between two marbles, polished plane, when their polished sides are parallel, and at a very small distance from one another. And if slender pipes of glass be dipped at one end into stagnating water, the water will rise up within the pipe; and the height, to which it rises, will be reciprocally proportional to the diameter of the cavity of the pipe, and will equal the height to which it rises between two planes of

gore, partes liquoris congelati ita coherent, ut corpus durum constituent. Atque experimento quidem jam memorato, de liquoribus in barometro ad tam insolitam uique altitudinem insensis, apparet partes corporum etiam fluidorum coherere inter se. Unde facile intelligi potest, quancumque demum causa efficiat ut partes Glaciei & Metallorum durorum cohaerant inter se, eandem tamen efficere, ut partes eorundem corporum etiam liquefactorum cohaerant; licet fortassis minus firmè. Nam particularum quidem corporum liquefactorum inter se huc perpetuò subleventur.

In the second *English*, and all the succeeding editions, the paragraph appeared as we now give it.

(^{ff}) *And of the same kind with these experiments, &c.*] Nothing of all that follows to the words, *philosophy to find them out*, was in the first *Latin* of Dr. Clarke. But the whole first appeared in the second *English*.

glass,

glass, if the femidiameter of the cavity of the pipe be equal to the distance between the planes, or thereabouts. And these experiments succeed after the same manner *in Vacuo* as in the open air (as hath been tried before the Royal Society) and therefore are not influenced by the weight or pressure of the atmosphere.

And if a large pipe of glass be filled with sifted ashes, well pressed together in the glass, and one end of the pipe be dipped into stagnating water; the water will rise up slowly in the ashes, so as in the space of a week or fortnight to reach up within the glass, to the height of 30 or 40 inches above the stagnating water. And the water rises up to this height by the action only of those particles of the ashes, which are upon the surface of the elevated water; the particles which are within the water, attracting or repelling it as much downwards as upwards. And therefore the action of the particles is very strong. But the particles of the ashes being not so dense and close together as those of glass, their action is not so strong as that of glass; which keeps quicksilver suspended to the height of 60 or 70 inches, and therefore acts with a force, which would keep water suspended to the height of above 60 feet.

By the same principle a sponge sucks in water; and the glands in the bodies of animals, according to their several natures and dispositions, suck in various juices from the blood.

If two plane polished plates of glass three or four inches broad, and twenty or twenty-five long, be laid, one of them parallel to the horizon, the other upon the first, so as at one of their ends to touch one another, and contain an angle of about 10 or 15 minutes; and the same be first moistened, on their inward sides, with a clean cloth dipped into oil of oranges or spirit of turpentine; and a drop or two of the oil or spirit be let fall upon the lower glass at the other end; so soon as the upper glass is laid down upon the lower, so as to touch it at one end, as above, and to touch the drop at the other end, making with the lower glass an angle of about 10 or 15 minutes; the drop will begin to move towards the concurrence of the glasses, and will continue to move with

(88) That is, a weight of 547 or 820 *lbs.* or upon an average $5\frac{1}{2}$ *cwt.* taking the weight of a cubic

with an accelerated motion, till it arrives at that concurrence of the Attractions. glasses. For the two glasses attract the drop, and make it run that way, towards which the attractions incline. And if, when the drop is in motion, you lift up that end of the glasses where they meet, and towards which the drop moves, the drop will ascend between the glasses, and therefore is attracted. And as you lift up the glasses more and more, the drop will ascend slower and slower, and at length rest; being then carried downward by its weight, as much as upwards by the attraction. And by this means you may know the force, by which the drop is attracted at all distances from the concurrence of the glasses.

Now by some experiments of this kind (made by Mr. *Hare*) it has been found that the attraction is almost reciprocally in a duplicate proportion of the distance of the middle of the drop from the concurrence of the glasses, viz. reciprocally in a simple proportion, by reason of the spreading of a drop, and its touching each glass in a larger surface; and again reciprocally in a simple proportion, by reason of the attractions growing stronger within the same quantity of attracting surface. The attraction therefore, within the same quantity of attracting surface, is reciprocally as the distance between the glasses. And therefore where the distance is exceeding small, the attraction must be exceeding great. By the Table in the second Part of the second Book, wherein the Thickness of coloured plates of water between two glasses are set down, the Thickness of the plate, where it appears very black, is three-eighths of the ten hundredth thousandth part of an inch. And where the oil of oranges between the glasses is of this thickness, the attraction, collected by the foregoing rule, seems to be so strong, as within a circle of an inch in diameter, to suffice to hold up a weight equal to that of a cylinder of water of an inch in diameter, and two or three furlongs in length (88). And where it is of a less thickness, the attraction may be proportionally greater, and continue to increase, until the thickness do not exceed that of a single particle of the oil. There are therefore agents in Nature able to make the par-

cubic foot of water, as it is stated in the Principia, Lib. II. Sect. VIII. Schol.

Elective

ticles of bodies stick together by very strong attractions. And it is the business of Experimental Philosophy to find them out.

Now the small particles of matter may cohere by the strongest attractions, and compose bigger particles of weaker virtue; and many of these may cohere and compose bigger particles, whose virtue is still weaker; and so on for divers successions, until the progression end in the biggest particles, on which the operations in chemistry, and the colours of Natural bodies depend; and which, by adhering, compose bodies of a sensible magnitude. If the body is compact, and bends or yields inward to pressure without any sliding of its parts, it is Hard and Elastic, returning to its figure with a force rising from the mutual attraction of its parts. If the parts slide upon one another, the body is Malleable or Soft. If they slip easily, and are of a fit size to be agitated by heat, and the heat is big enough to keep them in agitation, the body is Fluid; and if it be apt to stick to things, it is Humid; and the drops of every Fluid affect a round figure, by the mutual attraction of their parts, as the globe of the earth and sea affects a round figure, by the mutual attraction of its parts by gravity.

Since metals dissolved in acids attract but a small quantity of the acid, their Attractive force can reach but to a small distance from them. And as in Algebra, where Affirmative quantities vanish and cease, there Negative ones begin; so in Mechanicks, where Attraction ceases, there a Repulsive virtue ought to succeed. And that there is such a virtue, seems to follow from the reflexions and inflexions of the rays of light. For the rays are repelled by bodies, in both these cases, without the immediate contact of the reflecting or inflecting body. It seems also to follow from the emission of light; the ray, so soon as it is shaken off from a shining body by the vibrating motion of the parts of the body, and gets beyond the reach of attraction, being driven away with exceeding great velocity. For that force, which is sufficient to turn it back in reflexion, may be sufficient to emit it. It seems also

^{bb} — quantity for quantity.] Here followed in Dr. Clarke's first Latin. Ex eo, quod particulae Aeris à se invicem & à corporibus densis recedere conentur, fit etiam, ut Aer rarior sit in tubis vitreis exilibus, quam in amplioribus spatii; & per raritatem illam vi minore, quam apertus Aer,

also to follow from the production of air and vapour: the particles, when they are shaken off from bodies by heat or fermentation, so soon as they are beyond the reach of the attraction of the body, receding from it, and also from one another, with great strength; and keeping at a distance, so as sometimes to take up above a million of times more space than they did before, in the form of a dense body. Which vast contraction and expansion seems unintelligible, by feigning the particles of air to be springy and ramous, or rolled up like hoops, or by any other means than a Repulsive power. The particles of fluids, which do not cohere too strongly, and are of such a smallness as renders them most susceptible of those agitations which keep liquors in a *fluor*, are most easily separated and rarified into vapour; and in the language of the chemists, they are volatile, rarifying with an easy heat, and condensing with cold. But those which are grosser, and so less susceptible of agitation, or cohere by a stronger attraction, are not separated without a stronger heat, or perhaps not without fermentation. And these last are the bodies, which chemists call Fixed, and being rarified by fermentation, become true permanent air: those particles receding from one another with the greatest force, and being most difficultly brought together, which upon contact adhere most strongly. And because the particles of permanent air are grosser, and arise from denser substances than those of vapours; thence it is that true air is more ponderous than vapour, and that a moist atmosphere is lighter than a dry one, quantity for quantity^{bb}. From the same Repelling power it seems to be, that flies walk upon the water without wetting their feet; and that the object-glasses of long telescopes lie upon one another without touching; and that dry powders are difficultly made to touch one another so as to stick together, unless by melting them, or wetting them with water, which by exhaling may bring them together; and that two polished marbles, which by immediate contact stick together, are difficultly brought so close together as to stick.

Aer, premit superficiem aquae in quam inferior tubuli demersa sit extremitas; & doque permittat ut aqua illac in tubulam ascendat: quæ est Filtrationis causa, uti olim explicavit D. Hookius. Porro eadem vi repellenti tribuendum videntur, quod Muscæ, &c.

Necessity of
active princi-
ples

And thus Nature will be very conformable to herself, and very simple; performing all the great motions of the heavenly bodies by the attraction of gravity, which intercedes those bodies; and almost all the small ones of their particles, by some other Attractive and Repelling powers, which intercede the particles. The *vis inertiae* is a passive principle, by which bodies persist in their motion or rest; receive motion in proportion to the force impressing it, and resist as much as they are resisted. By this principle alone there never could have been any motion in the world. Some other principle was necessary for putting bodies into motion; and now they are in motion, some other principle is necessary for conserving the motion. For from the various composition of two motions, it is very certain that there is not always the same Quantity of motion in the world. For if two globes, joined by a slender rod, revolve about their common center of gravity with an uniform motion, while that center moves on uniformly in a right line drawn in the plane of their circular motion; the sum of the motions of the two globes, as often as the globes are in the right line described by their common center of gravity, will be bigger than the sum of their motions, when they are in a line perpendicular to that right line ⁽ⁱⁱ⁾. By this instance it appears, that motion may be got or lost. But by reason of the tenacity of fluids, and attrition of their parts, and the weakness of elasticity in solids, motion is much more apt to be lost than got, and is always upon the decay. For bodies, which are either absolutely hard, or so soft as to be void of elasticity, will not rebound from one another. Impenetrability makes them only stop. If two equal bodies meet directly *in Vacuo*, they will, by the laws of motion, stop where they meet, and lose all their motion, and remain in rest; unless they be Elastick, and receive new motion from their spring. If they have so much elasticity as suffices to make them rebound with a quarter, or half, or three-quarters of the force with which they come together, they will lose three-quarters, or half, or a quarter of their motion. And this may be

⁽ⁱⁱ⁾ The contrary seems to be true; that the sum of the motions will be the greatest, when the rod connecting the revolving bodies is perpendicular to the right line, along which the common center of gravity is moved. But in either way the different quantity of that sum of motion, in

be tried, by letting two equal pendulums fall against one another from equal heights. If the pendulums be of lead or soft clay, they will lose all, or almost all, their motions: if of Elastick bodies, they will lose all but what they recover from their Elasticity ^(kk). If it be said, that they can lose no motion, but what they communicate to other bodies; the consequence is, that *in Vacuo* they can lose no motion, but when they meet, they must go on and penetrate one another's dimensions ^(kk). If three equal round vessels be filled, the one with water, the other with oil, the third with molten pitch, and the liquors be stirred about alike to give them a vortical motion; the pitch, by its tenacity, will lose its motion quickly; the oil, being less tenacious, will keep it longer; and the water, being less tenacious, will keep it longest, but yet will lose it in a short time. Whence it is easy to understand, that if many contiguous *vortices* of molten pitch were each of them as large as those, which some suppose to revolve about the sun and fixed stars; yet these, and all their parts, would, by their tenacity and stiffness, communicate their motion to one another, till they all rested among themselves. *Vortices* of oil or water, or some fluid matter, might continue longer in motion; but unless the matter were void of all tenacity and attrition of parts, and communication of motion (which is not to be supposed) the motion would constantly decay. Seeing therefore the variety of motion, which we find in the world, is always decreasing; there is a necessity of conserving and recruiting it by active principles: such as are the cause of gravity, by which planets and comets keep their motions in their orbs, and bodies acquire great motion in falling; and the cause of fermentation, by which the heart and blood of animals are kept in perpetual motion and heat; the inward parts of the earth are constantly warmed, and in some places grow very hot; bodies burn and shine; mountains take fire; the caverns of the earth are blown up; and the sun continues violently hot and lucid, and warms all things by his light. For we meet with very little motion in the world, besides what

in these two positions of the rod, equally makes for our Author's assertion. Of which, perhaps, there is yet a more striking proof in the prodigious generation of motion by the collision of Elastic Bodies in certain arrangements. Vid. Hugen. *De Motu Corporum ex percussione*.

^(kk) If it be said—*Dimensions*.] Nothing answering to this sentence in Dr. Clarke's first Latin.

is owing to these active principles⁽¹⁾. And if it were not for these principles, the bodies of the earth, planets, comets, sun, and all things in them, would grow cold and freeze, and become inactive masses; and all putrefaction, generation, vegetation and life would cease, and the planets and comets would not remain in their orbs.

Of the Creation of Matter.

All these things being considered, it seems probable to me, that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable^(mm) particles; of such sizes and figures, and with such other properties, and in such proportion to space⁽ⁿⁿ⁾, as most conduced to the end for which he formed them; and that these primitive particles being solids, are incomparably harder than any Porous bodies compounded of them; even so very hard, as never to wear or break in pieces: no ordinary power being able to divide what God himself made One, in the first creation. While the particles continue entire, they may compose bodies of one and the same nature and texture in all ages: but should they wear away, or break in pieces, the nature of things, depending on them, would be changed. Water and earth, composed of old worn particles and fragments of particles, would not be of the same nature and texture now, with water and earth composed of entire particles, in the beginning. And therefore that nature may be lasting, the changes of corporeal things are to be placed only in the various separations, and new associations, and motions of these permanent particles; compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together, and only touch in a few points.

Principles of Motion.

It seems to me farther, that these particles have not only a *Vis inertiae*, accompanied with such Passive laws of motion as naturally result from that force; but also that they are moved by certain Active principles, such as is that of gravity, and that which causes fermentation, and the cohesion of bodies. These principles

⁽¹⁾ — *these active principles.*] In the first *Latin* of Dr. Clarke the sentence stood thus. Nam admodum paulum motus in Mundo invenimus, præterquam quod vel ex his Principiis actiuosis, vel ex imperio Voluntatis, manifesto oritur. The next sentence, *And if it were not for these principles—not remain in their orbs*, does not appear at all in the first *Latin*.

^(mm) — *impenetrable, moveable*] *Impenetrabiles, inertes & mobiles.* Dr. Clarke's first *Latin*.

⁽ⁿⁿ⁾ — *to space.*]—*coque numero et quantitate pro ratione spatii in quo futurum erat ut moverentur.*

ples I consider not as Occult qualities, supposed to result from the specifick forms of things, but as general laws of Nature, by which the things themselves are formed: their truth appearing to us by phænomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult^(oo). And the *Aristotelians* gave the name of Occult qualities not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to be the unknown causes of manifest effects: such as would be the causes of gravity, and of Magnetick and Electric attractions, and of fermentations, if we should suppose that these forces, or actions, arose from qualities unknown to us, and incapable of being discovered and made manifest. Such Occult qualities put a stop to the improvement of Natural Philosophy, and therefore of late years have been rejected. To tell us, that every species of things is endowed with an occult specifick quality, by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three general principles of motion from phænomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered: and therefore I scruple not to propose the principles of motion above-mentioned, they being of very general extent, and leave their causes to be found out^(pp).

Now by the help of these principles, all material things seem to have been composed of the hard and solid particles above-mentioned; variously associated, in the first creation, by the counsel of an intelligent Agent. For it became Him who created them, to set them in order. And if he did so, it is unphilosophical to seek for any other origin of the world, or to pretend that it might arise out of a chaos by the mere laws of Nature; though being once formed, it may continue by those laws for many ages. For

An intelligent Creator.

verentur. Dr. Clarke's first *Latin*; and so the sentence stands in Dr. Clarke's second *Latin* edition, which in most places was so corrected as to agree exactly with the second *English*. And to make sense of the passage, something is evidently wanting here to answer to the words of the *Latin*, *in quo futurum erat ut moverentur*. For to speak of particles of matter as bearing proportion to space indistinctly, were absurd.

^(oo) *And the Aristotelians—have been rejected.*] Not in Dr. Clarke's first *Latin*.

^(pp) *and leave their causes to be found out.*] Not in Dr. Clarke's first *Latin*.

while comets move in very excentrick orbs in all manner of positions, blind Fate could never make all the planets move one and the same way in orbs concentrick, some inconsiderable irregularities excepted, which may have risen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system wants a reformation. Such a wonderful uniformity in the planetary system must be allowed the effect of choice. And so must the uniformity in the bodies of animals, they having generally a right and a left side shaped alike, and on either side of their bodies two legs behind, and either two arms, or two legs, or two wings before upon their shoulders; and between their shoulders a neck⁽⁹⁹⁾ running down into a backbone, and a head upon it; and in the head two ears, two eyes, a nose, a mouth, and a tongue, alike situated. Also the first contrivance of those very artificial parts of animals, the eyes, ears, brain, muscles, heart, lungs, midriff, glands, larynx, hands, wings, swimming bladders, natural spectacles, and other organs of sense and motion; and the instinct of brutes and insects, can be the effect of nothing else than the wisdom and skill of a powerful ever-living Agent; who, being in all places, is more able by his will to move the bodies within his boundless uniform sensorium⁽¹⁰⁰⁾, and thereby to form and reform the parts of the universe, than we are by our will to move the parts of our own bodies⁽¹⁰¹⁾.

God not the
Soul of the
World.

And yet we are not to consider the world as the body of God, or the several parts thereof as the parts of God. He is an uniform Being, void of organs, members or parts; and they are his creatures subordinate to him, and subservient to his will; and he is no more the soul of them, than the soul of a man is the soul of the species of things carried through the organs of sense into the place of its sensation, where it perceives them by means of its immediate presence, without the intervention of any third

⁽⁹⁹⁾ — running down into a backbone.] Not in the first Latin of Dr. Clarke.

⁽¹⁰⁰⁾ — his boundless uniform sensorium]—infinito suo sensorio. Dr. Clarke's first Latin.

⁽¹⁰¹⁾ — than we are by our will to move the parts of our own bodies.]—quam Anima nostra, quæ est in nobis imago Dei, voluntate suâ ad corporis nostri membra movenda valet. Dr. Clarke's first Latin.

⁽¹⁾ And yet we are not to consider—things themselves.] Not in Dr. Clarke's first Latin.

⁽²⁾ — and in several proportions to space.]—vario quoque numero & quantitate pro ratione spatii

third thing. The organs of sense are not for enabling the soul to perceive the species of things in its sensorium, but only for conveying them thither; and God has no need of such organs, he being every where present to the things themselves⁽¹⁾. And since space is divisible in *infinitum*, and matter is not necessary in all places, it may be also allowed, that God is able to create particles of matter of several sizes and figures, and in several proportions to space⁽²⁾, and perhaps of different densities^(xx) and forces, and thereby to vary the laws of Nature, and make worlds of several sorts in several parts of the universe. At least, I see nothing of contradiction in all this.

As in Mathematicks, so in Natural Philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, ^(yy) and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments, or other certain truths. For hypotheses are not to be regarded in Experimental Philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from experiments; it may then begin to be pronounced, with such exceptions as occur. By this way of analysis we may proceed from compounds to ingredients; and from motions to the forces producing them; and in general, from effects to their causes; and from particular causes to more general ones, till the argument end in the most general. This is the method of Analysis. And the Synthesis consists in as-

spatii in quo insunt. Dr. Clarke's first and second Latin.

^(xx) How the primordial atoms, perfectly simple and perfectly solid, i. e. without pore, should differ in density, I cannot understand. It is true, different figures and sizes of the first solid atoms may produce very different densities of the compounds severally formed from them, though in the same degree of composition. Perhaps this might be our Author's meaning.

^(yy) — and in drawing general conclusions.] Of all that follows to the words *Exceptions as occur*, there was nothing in the first Latin of Dr. Clarke.

suming

fuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations.

In the two first Books of these Opticks, I proceeded by this analysis to discover and prove the original differences of the rays of light in respect of refrangibility, and colour; and their alternate fits of easy Reflexion and easy Transmission; and the properties of bodies, both opaque and pellucid, on which their reflexions and colours depend. And these discoveries being proved, may be assumed in the method of composition for explaining the phenomena arising from them: an instance of which method I gave in the end of the first Book. In this third Book I have only begun the analysis of what remains to be discovered about light, and its effects upon the frame of nature; hinting several things about it, and leaving the hints to be examined and improved by the farther experiments and observations of such as are inquisitive. And if Natural Philosophy in all its parts, by pursuing this method, shall at length be perfected; the bounds of Moral Philosophy will be also enlarged. For so far as we can know by Natural Philosophy what is the First cause, what power he has over us, and what benefits we receive from him; so far our duty towards him, as well as that towards one another, will appear to us by the light of Nature. And, no doubt, if the worship of false gods had not blinded the heathen, their Moral Philosophy would have gone farther than to the four Cardinal Virtues. And instead of teaching the transmigration of souls, and to worship the sun and moon, and dead heroes; they would have taught us to worship our true Author and Benefactor, as their ancestors, did under the government of *Noah* and his sons, before they corrupted themselves (^{zz}).

(^{zz}) — as their ancestors—themselves.] Not in the first *Latin* of Dr. *Clarke*, nor in the second *English*. In Dr. *Clarke's* second *Latin* the concluding sentence stands thus. Quod quidem fecerunt majores ipsorum, antequam animum moresque suos corruerant. Lex enim moralis ab origine gentibus universis erant septem illa Nonchidarum præcepta: quorum præceptorum primum erat, unum esse agnoscendum summum Dominum Deum, ejusque cultum non esse in alios transferendum. Etenim sine hoc principio nihil esset virtus aliud nisi merum nomen. In the third *English* the final sentence appeared as we now give it.

Fig 1.

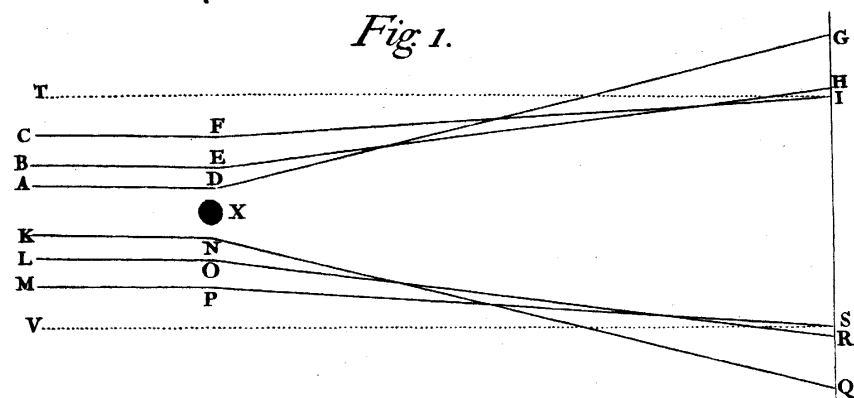


Fig 2.

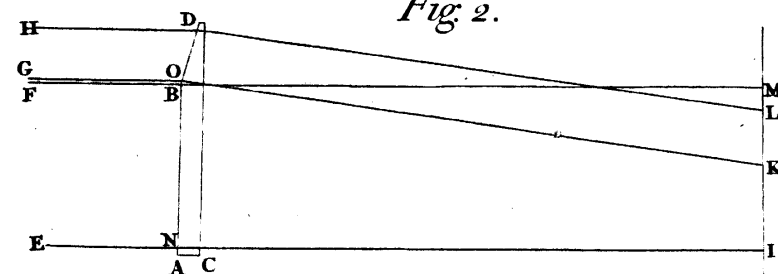
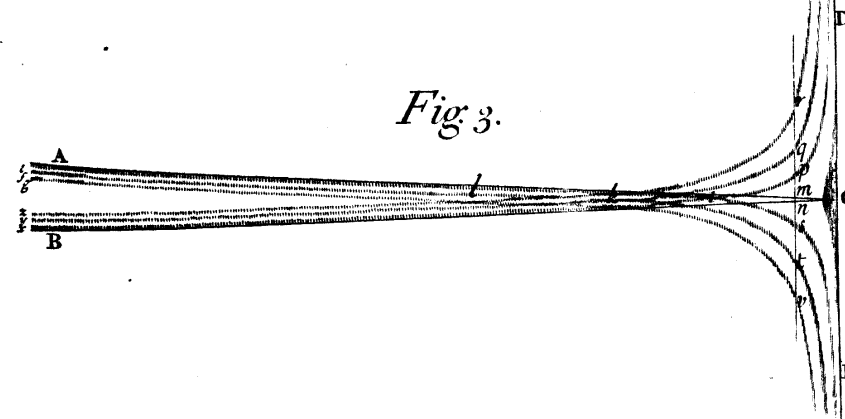


Fig 3.



L E T T E R S
O F
S I R I S A A C N E W T O N
O N

VARIOUS SUBJECTS IN NATURAL PHILOSOPHY.

N O W F I R S T

PUBLISHED AT LENGTH FROM THE ORIGINAL PAPERS EXTANT

I N T H E

ARCHIVES OF THE ROYAL SOCIETY OF LONDON.

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L E T T E R S, &c.

RELATING TO

REFLECTING TELESCOPES.

I.

To Mr. O L D E N B U R G.

SIR,

Cambridge,
March 16, 1678.

THE book, which my carrier by forgetfulness disappointed me of the last week, I have now received, and thank you for it. With the telescope which I have made, I have sometimes seen objects, and particularly the moon, very distinct in those parts of it, which were near the sides of the visible angle. And at other times, when it hath been otherwise put together, it hath exhibited things, not without some confusion. Which difference I attributed chiefly to some imperfection, that might possibly be, either in the figures of the metals or eye-glasses; and once I found it caused by a little tarnishing of the metal, in 4 or 5 days of moist weather.

One of the fellows of our College is making such another telescope, with which last night I looked on Jupiter; and he seemed as distinct and sharply defined, as I have seen him in other telescopes. When he hath finished it, I will examine more strictly, and send you an account of its performances. For it seems to be something better than that which I made. Yours, &c.

II.

To Mr. O L D E N B U R G.

SIR,

Cambridge,
March 19, 1678.

IN my last letter I gave you occasion to suspect, that the instrument which I sent you is, in some respect or other, indisposed;
or

or that the metals are tarnished. And by your letter of *March* 16, I am fully confirmed in that opinion. For whilst I had it, it represented the moon in some parts of it as distinctly, as other telescopes usually do, which magnify as much as that. Yet I very well know, that that instrument hath its imperfections, both in the composition of the metal, and in its being badly cast; as you may perceive by a scabrous place near the middle of the metal on the polished side, and also in the figure of the metal near that scabrous place. And in all those respects, that instrument is capable of further improvement.

You seem to intimate, that the proportion of 38 to 1 holds only for its magnifying objects at small distances. But if for such distances, suppose 500 feet, it magnify at that rate, by the rules of Optics it must, for the greatest distance imaginable, magnify more than $37\frac{1}{4}$ to 1; which is so inconsiderable a diminishing, that it may be even then as 38 to 1.

Here is made another instrument like the former, which does very well. Yesterday I compared it with a six-foot telescope; and found it not only to magnify more, but also more distinctly. And to-day I found, that I could read in one of the Philosophical Transactions, placed in the sun's light, at an hundred foot distance; and that at an hundred and twenty foot distance, I could discern some of the words. When I made this trial, its aperture, defined next the eye, was equivalent to more than an inch and a third part of the object metal. This may be of some use to those, that shall endeavour any thing in reflexions; for hereby they will in some measure be enabled to judge of the goodness of their instruments. I am, &c.

III.

ISAACI NEWTONI Matheſeos in Acad. Cantab. Prof. INVENTUM
Novum quo Teſcopia longa inſigniter contrahantur citra Eſ-
fecti fraudes. Exhibitum. Soc. Reg. Lond. 167 $\frac{1}{2}$.*

AB Speculum concavum metallicum, fundo Tubi adhærens;
cujus radius 14 digit. Anglicor.

CD

* This description was drawn up by Mr. Oldenburg, by order of the Royal Society, to be transmitted

CD speculum metallicum planum ovale, bacillo ferreo agglutinaturn, et circulo æneo, intra tubi cavitatem mobili, infixum.

F lens vitrea, cujus latus superius planum, inferius convexum, radius autem circa digiti semissis.

GGGG pars tubi interior, circulo æreo HI firmiter constricta, ita ut non facile moveri possit.

POKL pars tubi posterior, circulo æneo, PQ, immobiliter infixa. O uncus ferreus, circulo æneo PQ affixus, ultra tubi axem extensus; cui clavus cochleatus N immixtus tubi partem posteriorem antrorsum pellit, vel retrahit, ad speculorum debitam distantiam investigandam, parte priore fixa remanente.

NMGI ferrum curvatum, quod tubum sustinet; globo ligneo versatili S, clavo R affixum.

Centrum speculi plani, CD, locatur in axe Tubi; ita ut in ipsum perpendicularis, a centro lentis demissa, cum axe angulum rectum constituat; et objecti species, a speculo concavo in idem repercussa, versus lentis focus E reflectatur.

Corona ventilogio ornamenti gratia imposita, 300 circiter pedibus distans, cum uno oculo huic tubo admoto spectabatur altero in chartam subjectam, 11 circiter digitis ab ipso distantem, magnitudine et figura A insignita apparebat. Tubus autem vulgaris 25 digit. longus, lente objectivâ convexâ, oculari verò utrinque concavâ (cujus radius 2. digit est) oculo admoto, figuram coronæ magnitudine B, eadem observatâ chartæ ab oculo distantia, alteri oculo exhibebat, consentientibus post repetita experimenta DD. Brounker, Wren, Hook.

IV.

To Mr. O L D E N B U R G.

Sir,

Cambridge,
Jan, 6, 167 $\frac{1}{2}$.

THE description of the instrument you sent is very well; only the radius of the concave metal, which you put 14 inches, is more justly $12\frac{2}{3}$ or 13 inches; and the radius of the eye-glass, which you put half an inch, is the twelfth part of it, if not less: for the metal collects the sun's rays at 6 $\frac{1}{2}$ inches distance, and the transmitted to Mr. Hugen at Paris. The Society judging this the best method of securing the fame of this great invention to the Author.

eye-

eye-glass is less than $\frac{1}{6}$ of an inch distance from its vertex. By the tools also to which they were ground, I know their dimensions; and particularly measuring the diameter of the hemispherical concave, in which the eye-glass was ground, I find it the sixth part of an inch.

Perhaps it may give some satisfaction to Mr. *Hugens* to understand, in what degree it represents things distinct, and free from colours; and to know the aperture by which it admits light. And after the words [*versus focum reflectatur*] it may not be amiss to add this note.

Conferendo distantias foci istius à verticibus lentis et speculi concavi, hoc est EF $\frac{1}{6}$ dig. et ETV $6\frac{1}{3}$ dig. prodit ratio 1 ad 38; quâ judicatur objecta 38 vicibus circiter ampliari.

And to this proportion is very consentaneous the observation of the crown on the weather-cock. For the scheme represents it bigger by $2\frac{1}{2}$ times, when seen through this, than when through an ordinary perspective. And so supposing that to magnify 13 or 14 times (as by the description it should) this by experiment proportionably must magnify almost as much as I have assigned it.

To the objection, that with it objects are difficultly found; I may answer, that that is the inconvenience of all tubes that magnify much. And that after a little use, the inconvenience will grow less; for I could readily enough find any day objects, by knowing which way they were posited from other objects, that I accidentally saw in it. But in the night to find stars, I confess is troublesome enough; yet this may be easily remedied by two sights affixed to the iron rod, by which the tube is sustained; and such I once intended should have been made, before I sent it away from me; but that I thought the defect would not be judged material. If such sights be not found a sufficient remedy, there may be an ordinary prospective glass fastened to the same frame with the tube, and directed towards the same object, as *Des Cartes* in his *Dioptrics* hath described, for remedying the same inconvenience of his best telescopes.

The plane side of the eye-glass is apt to be soiled with the dust falling down upon it. And therefore the little leaden ring, put into the orifice of the bigger leaden barrel to moderate its aperture, must be sometimes taken out, and the glass wiped with
3 leather

leather done upon the small end of a stick, or other such like contrivance. But care must be taken, that the said ring be not lost; for without it objects appear very confused, at the edges of the apparent space. So if the concave metal contract any dullness, by moisture or otherwise; it ought to be taken out and rubbed with gentle leather, but not with putty, or any thing that may wear the metal.

I am very sensible of the honour done me by the bishop of Sarum, in proposing me a candidate, and which I hope will be further conferred upon me by my election into the Society. And if so, I shall endeavour to testify my gratitude, by communicating what my poor and solitary endeavours can effect, towards promoting the Philosophical design. I am, &c.

V.

To Mr. OLDENBURG.

SIR,

Cambridge,
Jan. 18, 1671.

UNDERSTANDING, by your last, that some of the fellows of the Hon. Society, in order to a bigger reflective Telescope, are devising a fit metalline matter; let me presume to give them this caution, that whilst they seek for a white, hard and durable metalline composition, they resolve not upon such an one as is full of small pores, only discoverable by a microscope: for though such an one may, to appearance, take a good polish, yet the edges of those small pores will wear away faster in the polishing, than the other parts of the metal; and so, however the metal seem polite, yet it shall not reflect with such an accurate regularity as it ought to do. Thus tin-glass, mixed with ordinary bell-metal, makes it more white, and apt to reflect a greater quantity of light; but withal, its fumes raised in the fusion, like so many aerial bubbles, fill the metal full of the microscopical pores. But white arsenick both blanches the metal, and leaves it solid, without any such pores; especially if the fusion hath not been too violent. What the stellate regulus of *Mars* (which I have sometimes

VOL. IV.

N n

times

Composition times used) or rather such like substance, will do, deserves particular examination.

Let me add further this intimation; that putty, or other such like powder, with which it is polished, by the sharp angles of its particles, fretteth the metal, if it be not very fine, and filleth it full of such small holes as I speak of. Wherefore care must be taken of that, before judgment be given, whether the metal be, throughout the body of it, porous or not.

I desire, that in your next letter you would inform me, for what time the Society continue their weekly meetings; because if they continue them for any time, I am purposing them, to be considered of and examined, an account of a Philosophical discovery, which induced me to the making of the said Telescope; and I doubt not but will prove much more grateful than the communication of that instrument; being in my judgment the oddest, if not the most considerable detection, which hath hitherto been made in the operations of Nature.

VI.

To Mr. OLDENBURG.

SIR,

Cambridge,
Jan. 29, 1671.

NOT having tried many proportions of the arsenick and metal, I do not affirm which is absolutely best, but think there may conveniently be used any quantity of arsenick equalling in weight between a sixth and an eighth part of the copper; a greater proportion making the metal brittle.

The way which I used is this: I first melted the copper alone, then put in the arsenick, which being melted, I stirred them a little together, bewaring, in the mean time, not to draw in breath near the pernicious fumes. After this I put in tin; and again, so soon as that was melted (which was very suddenly) I stirred them well together, and immediately poured them off.

I know not, whether by letting them stand longer on the fire after the tin was melted, a higher degree of fusion would have made the metal porous; but I thought that way I proceeded safest.

In

In that metal, which I sent to *London*, there was no arsenick, but a small proportion of silver; as I remember, one shilling in three ounces of metal. But I thought the silver did as much harm in making the metal soft, and so less fit to be polished, as good in rendering it white and luminous.

At another time I mixed arsenick one ounce, copper six ounces, and tin two ounces; and this an acquaintance of mine hath polished better than I did the other. I am, &c.

VII.

To Mr. OLDENBURG.

SIR,

Cambridge,
Feb. 20, 1671.

I Received your's February 17th. And having considered Mr. *Hook's* observations on my discourse, am glad that so acute an objector hath said nothing that can enervate any part of it. For I am still of the same judgment, and doubt not but that upon severer examinations it will be found as certain a truth, as I have asserted it. You shall very suddenly have my answer.

In Mr. *Hugenius's* letter there are several handsome and ingenious remarks: and what he says concerning the grinding of Parabolical Conoids by geometrical rules, I do with him despair of. But I doubt not but that the thing may be in some measure accomplished by mechanical devices. This is all at present from, yours, &c.

VIII.

To Mr. OLDENBURG.

SIR,

March 26, 1671.

ABOUT ten days since, at night, I saw a dull star south-west of *Perseus*, which I now take to have been that comet of which you gave me information; but it was very small, and had not any visible tail, which made me regard it no further, and I fear it will now be difficult to find it.

N n 2

Since

Performance
and imperfec-
tions of the
metalline
Reflector.

Since my last letter, I have further compared, and find that of metal to represent as well the moon as nearer objects, something distincter than the other. But I must tell you also, that I find that other (which I borrowed to make the comparison) to be none of the best in the kind, and therefore would not have you rely on the observations made with it, but rather estimate the performances of the metalline telescope by the distances of between 100 and 120 feet, at which I and others could read in the Transactions, as I found by measure; at which time the aperture was $1\frac{1}{3}$ of an inch, which I knew by trying, that an obstacle of that breadth was requisite to intercept all the light which came from one point of the object. I should tell you also, that the little plain piece of metal next to the glass is not truly figured, whereby it happens, that objects are not so distinct at the middle as at the edges. And, I hope, that by correcting its figure (in which I find more difficulty than one would expect) they will appear all over distinct and distinct in the middle than at the edges. And I doubt not, but then the performances will be greater. But yet I find, that there is more light lost by reflection of the metal I have hitherto used than by transmission through glasses, for which reason a shallower charge would probably do better for obscure objects, suppose such a one as would make it magnify 34 or 32 times; but of bright objects at any distance it seems capable of magnifying 38 or 40 times with sufficient distinctness; and for all objects the same charge, I believe, may be allowed, if the steely matter employed at London be more strongly reflective, than this which I have used.

Lengths,
apertures, and
charges.

The performances of one of these instruments of any length being known, it will appear, by this following Table, what may be expected from those of other lengths by this way, if art can accomplish what is promised by the theory. In the first column is expressed the length of the telescope in feet, which doubled, gives the semidiameter of the sphere on which the concave metal is to be ground. In the second columns are the proportions of the apertures for those several lengths; the third column are the proportions of the charges or diameters of the spheres on which

Lengths.	Apertures.	Charges.
$\frac{1}{2}$	100	100
1	168	119
2	283	141
3	383	157
4	476	168
5	562	178
6	645	186
8	800	200
10	946	211
12	1084	221
16	1345	238
20	1591	254
24	1824	263

which the convex superficies of the eye-glasses are to be ground. Lengths, apertures, and charges.

The use of this Table will best appear by example. Suppose, therefore, a half-foot telescope may distinctly magnify 30 times with an inch aperture, and be it required to know what ought to be the analogous constitution and performance of a four-foot telescope: by the

second column, as 100 to 476, so are the apertures; as also the numbers of times which they magnify. And consequently since the half-foot tube hath an inch aperture, and magnifyeth 30 times; a four-foot tube proportionally should have $4\frac{7}{100}$ inches aperture, and magnify 143 times. And by the third column, as 100 to 168, so are their charges. And therefore if the diameter of the convexity of the eye-glass for a half-foot telescope be $\frac{1}{2}$ of an inch, that for a four should be $\frac{168}{300}$; that is, about $\frac{1}{2}$ of an inch. In like manner if a half-foot telescope may distinctly magnify 36 times with $1\frac{1}{4}$ of an inch aperture, a four-foot telescope should with equal distinctness magnify 171 times with 6 inches aperture. And one of six foot should magnify 232 times with $8\frac{2}{3}$ inches aperture; and so of other lengths. But what the event will really be, we must wait to see determined by experience; only this I thought fit to insinuate, that they which intend to make trials in other lengths, may more readily know how to design their instruments. Thus for a four-foot tube, since the aperture should be 5 or 6 inches, there will be required a piece of metal 7 or 8 inches broad at least; because the figure will scarcely be true to the edges: and the thickness of the metal must be proportional to the breadth, least it bend in the grinding. The metals being polished, there may be trials made with several eye-glasses to find what charge may, with best advantage, be made use of. Thus much of these telescopes; and at present I shall trouble you no farther, than to thank you for your last intelligence, by which you have obliged, Sir,

&c.

IX.

To Mr. OLDENBURG.

SIR,

March 30, 1672.

Advantages of Reflectors.

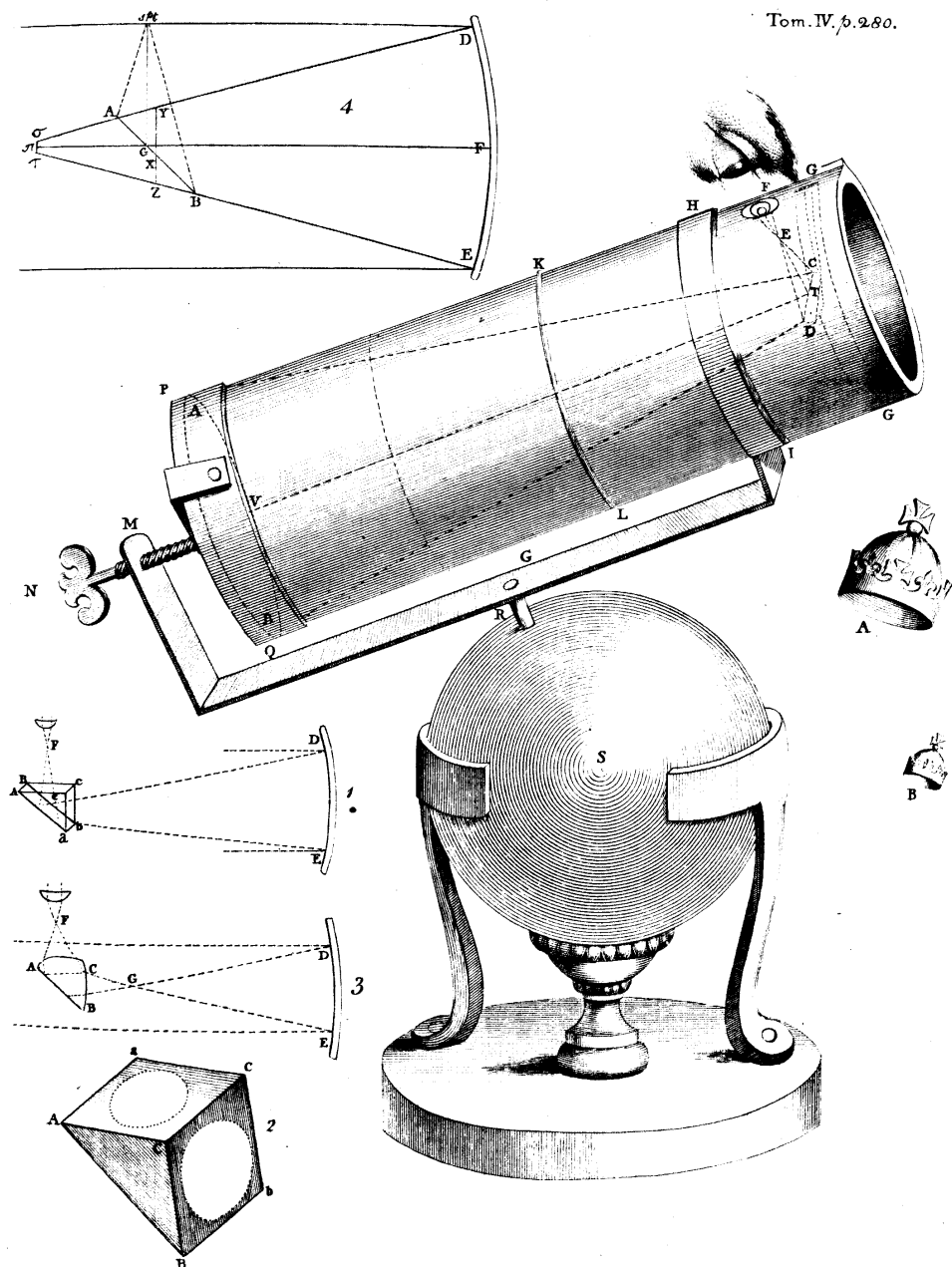
I Doubt not but Monf. *Auxout* will allow the advantage of reflections in the theory to be very great, when he shall be informed himself of the different refrangibility of the several rays of light; and for the pratique part, it is in some measure manifest, by the instruments already made, to what degree of vivacity and brightness a metalline substance may be polished: nor is it improbable, but that there may be new ways of polishing found out for metal, which will far excel those that are yet in use. And when a metal is once well polished, it will be a long while preserved from tarnishing, if diligence be used to keep it dry, and close shut up from the air. For the principal cause of tarnishing seems to be the condensing of moisture on its polished surface; which, by an acid spirit wherewith the atmosphere is impregnated, corrodes and rusts it; or at least at its exhaling, leaves it covered over with a thin skin, consisting partly of an earthy sediment of that moisture, and partly of the dust which, flying to and fro in the air, had settled and adhered to it. When there is not occasion to make frequent use of the instruments, there may be other ways to preserve the metals for a long time, as perhaps by immersing them in spirit of wine, or some other convenient liquor. And if they chance to tarnish, yet their polish may be recovered by rubbing them with a soft piece of leather, or other tender substance, without the assistance of any fretting powders, unless they happen to be rusty, for then they must be new polished. I am very sensible, that metal reflects less light than glass transmits; and for that inconvenience I gave you a remedy in my last letter, by assigning a shallower charge in proportion to the aperture than is used in other telescopes. But as I have found some metalline substances to be more strongly reflective, and to polish better, and to be freer from tarnishing than others, so I hope there may be in time found out some substance much freer from those inconveniences, than any yet known.

In the mean time, to remedy in some measure these inconveniences, I shall propound a way of using, instead of the little oval metal, a glass or chrytal figured like a triangular prism, as you see it represented in the first scheme by the figure A, B, C. Its side *ABba* I suppose to perform the office of that metal, by reflecting towards the eye-glass the light which comes from the concave *DE*, which light I suppose to enter into this prism at its side *cbbc* and *cbbc*; and lest any colours should be produced by the refraction of those planes, it is requisite that the angles of the prism at *aa* and *bb* be precisely equal. Which may be most conveniently performed by making them half right angles, and consequently the third angle at *cc* a right one. The plane *ABba*, without being foliated, will reflect all the light incident on it, especially if the prism be made of chrytal. But to exclude all unnecessary light, it is convenient that it be all over covered with some black substance, excepting two circular spaces of the planes *ac* and *bc* for the useful light to pass through, as you see it designed in the second Scheme. The length of this prism should be such, that its sides *ac* and *bc* may be four square; and so much of the angles *B* and *b* as are superfluous ought to be ground off, to give passage to as much light as is possible from the object to the concave. There is one very considerable advantage of this prism which the oval metal is not capable of, without using two eye-glasses, and it is that of its sides *acca* and *bccb* be ground convex lens. The manner you have expressed in the third Scheme, where suppose *G* to be the focus of the concave, and *F* of the eye-glass at which the rays cross twice before their arrival at the eye. But it is convenient, that the first trials be made with prisms whose sides are all of them plane. And thus much concerning Monf. *Auxout's* considerations.

To the queries of Monf. *Denys*, I answer, 1. That a tube of six inches is capable of bearing an aperture (limited next the eye) so large, that an obstacle of $1\frac{1}{4}$ or $1\frac{1}{2}$ of an inch in breadth, shall be requisite to intercept all the light coming from one point of the object towards the concave metal.

But it is convenient, that the tube be a little wider than that aperture Adjustment of the diameter

aperture precisely requires, suppose $1\frac{1}{2}$ or $1\frac{2}{3}$ of an inch, and not more; and the whole breadth of the metal should not be less than two inches, because its figure towards the edge will scarcely be so true as to be useful. And by that means it may be also conveniently fastened to the end of the tube on the outside, so at pleasure to be taken off, and laid up close from the air, to preserve it from tarnishing. How the diameter of the tube is to be enlarged according to its length, will appear by the table of apertures and charges which I sent you in my last letter of *March 28*; namely, the cube of its length should be proportional to the square of its diameter or aperture at the metal. So that the advantage of augmenting the length of tubes, is by this way far greater than by refractions, where their length ought to be proportional to the square of the diameter of the aperture. 2. The breadth, or shortest diameter of the little oval metal for a tube of six inches, should not be greater than $\frac{1}{3}$, nor less than $\frac{1}{4}$ of an inch. And the longest diameter should be to the shortest, as about 10 to 7. But you may more exactly determine these diameters for tubes of all lengths after this manner. In the fourth figure, let AB represent the oval set edgewise; DE, the concave; FG, its axis; GP, the reflex of that axis; $\pi\tau$, the diameter of the hole through which the light is transmitted to the eye, and p the center of that hole. Produce F, G to π , so that $G\pi$ may be equal to Gp ; erect $\pi\sigma$ and $\pi\tau$ equal to ps and pt ; and from σ and τ draw two lines, σD and τE , to the outmost parts of the concave within the tube, intersecting AB in A and B; and AB shall be the long diameter of the oval, which bisect in x ; and perpendicular to rx erect xy and xz , occurring with σd and τe in y and z ; and a mean proportional between xy and xz , doubled, shall be the other short diameter: for by viewing the Scheme you will easily perceive, that an oval, described with those rectangular conjugate diameters, is of sufficient bigness to reflect all the useful light towards the eye, if it be rightly placed in the tube; and a broader metal would not only intercept so many of the best rays, but some of the scattering light, reflected every way from its superfluous parts, would fall on the eye-glass, and make the object appear something confused, and



and as it were in a mist. This, Sir, is that which, in answer to your letter, my present thoughts suggest to your faithful servant,
&c.

X.

To Mr. O L D E N B U R G.

SIR,

May 4, 1672.

I Should be glad to meet with any improvement of the cata-
dioptrical telescope, but that design of it which, as you inform
me, M. *Casségrain* hath communicated three months since, and
now printed in the *French Journal*, I fear will not answer expect-
tation; for when I first applied myself to try the effects of re-
flections, Mr. *Gregory's Optica Promota* (printed in the year 1663)
being fallen into my hands, where there is an instrument de-
scribed (pag. 94) like that of Mr. *Casségrain*, with a hole in the
midst of the object-glass, to transmit the light to an eye-glass
placed behind it; I had thence an occasion of considering that
sort of constructions, and found their disadvantages so great, that
I saw it necessary, before I attempted any thing in the practice, to
alter the design of them, and place the eye-glass at the side of
the tube, rather than at the middle.

The disadvantages of it you will understand by these particulars: Disadvantages of the Gregorian Construction.
1. There will be more light lost in the metal, by reflexion from
the little convex speculum, than from the oval plane: for it is an
obvious observation, that light is most copiously reflected from any
substance when incident most obliquely. 2. The convex speculum
will not reflect the rays so truly as the oval plane, unless it be of an
hyperbolic figure, which is incomparably more difficult to form than
a plane; and, if truly formed, yet would only reflect those rays
truly which respect the axis. 3. The errors of the said convex
will be much augmented by the too great distance through which
the rays reflected must pass, before their arrival at the eye-glass;
for which reason, I find it convenient to make the tube no wider
than is necessary, that the eye-glass may be placed as near to the
oval plane as is possible, without obstructing any useful light in
its

its passage to your object-metal. 4. The errors of the object-metal will be more augmented by reflexion from the convex than from the plane, because of the inclination or deflexion of the convex on all sides from the points on which every ray ought to be incident. 5thly. For these reasons there is requisite an extraordinary exactness in the figure of the little convex; whereas I find by experience, that it is much more difficult to communicate an exact figure to such small pieces of metal, than to those that are greater. 6. Because the errors at the perimeter of the concave object-metal caused by the sphericity of its figure, are very much augmented by the little convex; it will not, without indistinctness, bear so large an aperture, as in the other construction. 7. By reason that the little convex conduces very much to the magnifying virtue of the instrument which the oval plane doth not, it will magnify much more in proportion to the sphere on which the great concave is ground, than in the other design. And so magnifying objects much more than it ought to do in proportion to its aperture, it must represent them very obscure and dark; and not only so, but also confused, by reason of its being overcharged. Nor is there any convenient remedy for this; for if the little convex be made of a larger sphere, that will cause a greater inconvenience, by intercepting too many of the best rays; or if the charge of the eye-glass be made so much shallower as is necessary, the angle of vision will thereby become so little, that it will be very difficult and troublesome to find an object; and of that object, when found, there will be but a very small part seen at once.

Disadvantages
of Casse-
grain's Con-
struction.

By this you may perceive, that the three advantages which Monf. *Cassegrain* propounds to himself are rather disadvantages; for, according to his design, the aperture of the instrument will be but small, the object dark and confused, and also difficult to be found. Nor do I see why the reflexion is more upon the same axis, and so more natural in one case than in the other, since the axis itself is reflected towards the eye by the oval plane. And the eye may be defended from external light as well at the side as at the bottom of the tube. You see, therefore,

that

that the advantages of this design are none; but the disadvantages so great and unavoidable, that I fear it never will be put in practice with good effect; and when I consider, that by reason of its resemblance with other telescopes, it is something more obvious than the other construction, I am apt to believe, that those who have attempted any thing in Catoptricks, have ever tried it in the first place; and that their bad success in that attempt hath been the cause why nothing hath been done in reflexions.

For Mr. *Gregory*, speaking of these instruments in the aforesaid book, p. 95, saith, *De mechanicâ horum speculorum et lentium, ab aliis frustra tentatâ, ego, in mechanicis minis versatus, nihil dico;* so that there have been trials made of these telescopes, but yet in vain. And I am informed, that about 7 or 8 years since, Mr. *Gregory* himself, at *London*, caused one of six foot to be made by Mr. *Reive*, which I take to be according to the aforesaid design described in his book, because though made by a very skillful artist, yet it was without success.

I could wish, therefore, M. *Cassegrain* had tried his design, before he divulged it; but if for further satisfaction he please hereafter to try it, I believe the success will inform him, that such projects are of little moment, till they be put in practice. I am, Sir, your humble servant,

&c.

XI.

To Mr. C O L L I N S.

SIR,

Dec. 10, 1672.

MR. *GREGORY* is pleased to consider further the most advantageous construction of cata-dioptrical telescopes. And as his design in his *Optica Promota* excels that of Mr. *Cassegrain* (though they differ so slightly, that I thought it not worth the while to take notice of the difference) the advantage being, that the little concave ellipsis comes nearer to a spherical figure than the convex hyperbola, so I conceive his present proposal excels them both; of making the speculum plane. And this I conjecture is the

Of Gregory's
second
Construction.

way which Signior *Salvetti*, one of the grand duke's musicians, mentioned in the last Transactions, intends to make experiment of; excepting that, instead of the convex eye-glass, he may probably substitute a concave one to erect the object. But yet I cannot think it the best; it being liable to the 1st, 3d and last of these difficulties urged against Mr. *Cassegrain*; and, in my judgment, not wholly capable of the advantages which Mr. *Gregory* propounds.

Advantages
of oblique
Reflection.

The first disadvantage was, that more light is lost in direct than oblique reflexion. I am convinced by several observations, that reflexion is not made by the solid parts of a body, as is commonly presumed, but by the confine of the two mediums, whereof one is within and the other without the body. And as stones are reflected by water, when thrown obliquely, which force their way into it, when thrown directly downwards; so the rays of light (whether corporeal like stones or not) are most easily and copiously reflected when incident most obliquely. This you may observe in the passage of light out of glass into air; which is reflected more and more copiously, as the obliquity is increased, until beyond a certain degree of obliquity it be wholly reflected. Also in the reflection of light by an imperfectly polished plate of brass or silver, or any other metal; you may observe, that the image of objects, which by direct reflexion appear dull and confused, appear by very oblique reflexion pretty distinct and vigorous. This advantage of oblique reflexion would be inconsiderable, if metal reflected almost all the light directly incident on it. But so far as I can observe, there is at least a third part, if not the better half, of the light lost and stifled in the metal at every reflexion; and it is of some estimation, if a third or fourth part of that may be redeemed, by setting the flat speculum obliquely. As for Mr. *Gregory's* insinuation, that direct rays have the advantage of oblique; because a direct ball is directed more regularly from a rough wall than an oblique one; if he please to consider how different are the causes and circumstances of those reflexions, possibly, upon second thoughts, he may apprehend why the contrary ought to happen

happen in light: at least the experiment of the rudely-polished plate of metal may persuade him.

The next disadvantage, arising from the distance of the little speculum from the eye-glass, being allowed; I pass to the last: which is to this effect. That if to diminish the magnifying virtues of the instrument, the little speculum be made of a larger sphere (as it is in Mr. *Gregory's* design, a plane being equivalent to a sphere whose center is indefinitely distant) that would cause too many of the best rays to be intercepted. And though in his design scarce a fourth part of the whole light be intercepted, yet those rays seem to me of more value than twice their number next the circumference of the tube, because they principally conduce to distinct vision. Their loss will be judged considerable by those, that have thought the loss of scarce the 40th part of the light in my way worthy of being objected, by reason that they were the best of the rays.

There are yet other considerations by which Mr. *Gregory's* tube may perhaps be thought less advantageous. As, that unless the speculum, F, be made so broad as to intercept more than a quarter, or perhaps than a third part of the whole light, it will be difficult to enlarge the aperture, as is requisite for viewing dull and obscure objects: that the eye-glass, if placed at the bottom, will scarcely be well defended from the unuseful glaring light, which in the day-time comes from objects on all sides the flat speculum; at least not so well as by setting it at the side; and that an artificer can scarcely polish the great concave so truly when perforated in the middle: for the metal near the hole will be apt to wear away too fast, as it doth near the exterior limb. And though the hole may be made after it is polished, yet if the figure happen to be less true, or if afterwards the metal chance to tarnish, it must be polished again.

As for the advantages propounded by Mr. *Gregory*, I see not why the first should be reckoned for one, viz. that the distance EF grows almost the one-half less, and therefore the errors of the concave CD are almost diminished upon the plane F by one-half. For how much those errors of the concave CD are increased or diminished, is to be estimated by the divarication of the rays,
not

not at the plane F, but at the focus of that concave CD. And there the errors in both cases will be alike, provided the speculum F be accurately plane. But if there be any irregularities in the figure of the speculum F, they will cause errors so much greater in one case than in the other, as that speculum is remoter from the eye-glass; which in large telescopes may be more than 15 or 20 inches.

Great advantage of the shortness of the Tube.

The other advantage, viz. that his tube will be little more than half the length of mine, I should allow to be very considerable; if I thought, that with equal art in the mechanism, it would be made to do the same effect. The greatest difficulty is in forming the great concave; which when once well done, perhaps it may be thought most advantageous to make the best use of it with a longer tube.

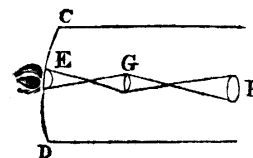
Convenience of varying the charge greater in the Newtonian Construction.

The supposed advantage of telescopes with convex or concave speculums, in that they may have any desirable charge, by altering the distances of the eye-glass and specula, agrees more conveniently with my design of the instrument, if that speculum be made use of, which I described in a letter to Mr. *Oldenburg*, in answer to Mr. *Hugens's* considerations on these subjects; which possibly you may have seen. For instance; to double the charge, the eye-glass in the other way must be drawn out almost as far behind the great concave as the little speculum is before it; whereby the length of the tube will be almost doubled. Whereas in my way it need be drawn out no farther from the side of the tube than a quarter of the tube's diameter. The charge may be also conveniently varied, by having 2 or 3 eye-glasses of several depths set in a girdle; any of which may be adjusted to the metal F by sliding the girdle about the tube, or by sliding the ring within the tube to which the metal F is fastened.

That telescopes with convex or concave speculums should be overcharged, is not necessary; but yet it is not avoidable without running upon one of the other two inconveniences, described in the 7th particular of my considerations on Mr. *Cassegrain's* tube, as I there intimated.

To diminish some of the aforesaid disadvantages, there may be still new variations or additions to these designs. As, for instance, by

by using two eye-glasses. Suppose CD represent the great concave; E, the eye-glass; and G, another double convex-glass, between E and F, on both sides of which the rays cross. This way of redoubling these tubes seems not inferior to the rest. For thus the object appears erect; the speculum F intercepts less light, and the charge may be varied at pleasure, only by changing the positions of G and F. But yet this is not without imperfections: and particularly, besides those common with the other designs, this glass G will intercept many of the best rays in their passage from the concave CD to the little speculum F; unless it be made less than is consistent with some other inconveniences. And by the iterated decussations of the rays, objects will be rendered less distinct; as is manifest in dioptric telescopes, where 2 or 3 eye-glasses are applied to erect the object.



As to the attempt in which Mr. *Reeves* was employed, I presumed it had been done with much more accurateness than Mr. *Gregory* now signifies: because Mr. *Hook*, who you know is a curious and accurate experimenter, affirms, in his considerations on my letter to Mr. *Oldenburg* concerning refractions and colours, published in the *Transactions*, No. 80, that he made several experiments with that instrument. And though he lays the blame on Mr. *Reeves's Encheiridia*, yet he says not that he blamed him then, when the experiment was made. His words are these; "I have made several trials both for telescopes and microscopes by reflexions, which I have mentioned in my *Micrography*; but deserted it as to telescopes, when I considered, that the focus of a spherical concave is not a point, but a line; and that the rays are less truly reflected to a point by a concave, than reflected by a convex. Which made me seek that by refraction, which I found could not be expected by reflexion. Nor, indeed, could I find any effect of it, by one of six-foot radius, which about 7 or 8 years since Mr. *Reeves* made for Mr. *Gregory*, with which I made several trials. But it now appears that it was for a want of a good *encheiridia*, from which cause

" many

“ many good experiments have been lost. Both which considerations discouraged me from attempting further that way; especially since I found the parabola much more difficult to execute than the hyperbola or ellipsis.”

From hence I might well infer, that the want of a good *encheiria* appeared not till now: and that Mr. *Hook* was discouraged from attempting further that way, only by these 2 or 3 considerations: that a convex, as he presumes, refracts more truly than a concave reflects: that he found no effect by one of six foot radius, which till now he attributed to some other cause than the want of a good *encheiria*; namely, to the supposed less true reflexion of a spherical concave: and that he apprehended a greater difficulty of describing a parabola than an hyperbola or ellipsis. Nor could I well interpret the cause, from which many good experiments have been lost, to have been other than the want of a good *encheiria*; which till afterwards appears not to have been wanting. I contend not, that this was Mr. *Hook's* meaning; but only that his words seemed to import thus much. Which gave me occasion to think, there was no diligence wanting in making the experiment; especially since he expresseth, that he made several trials with it.

And that you may not think I strained Mr. *Gregory's* sense, where he speaks of hyperbolic and elliptic glasses and speculums attempted in vain; I could ask, to what end those speculums were attempted, if not to compose optic instruments? Which is all I would infer from those words. For that these instruments, if at all attempted, were attempted in vain, is evident by the want of success.

This, Sir, I have said, not that I desire to discourage the trial of any practical way; or to contend with Mr. *Gregory* about so slender a difference. For I doubt not, but when he wrote his *Optica Promota*, he could have described more fashions than one of these telescopes, and perhaps have run through all the possible cases of them, if he had thought it worth his pains. Because Mr. *Cassegrain* propounded his supposed invention pompously, as if the main business was in the contrivance of these instruments; I thought fit to signify, that that was none of his

contrivance,

contrivance, nor so advantageous as he imagined. And I have now sent you these further considerations on Mr. *Gregory's* answer, only to let you see, that I chose the most easy and practicable way to make the first trials. Others may try other ways. Nor do I think it material which way these instruments are perfected, so they be perfected.

XII.

Mr. NEWTON'S answer to a letter of Mr. GREGORY'S, dated March 7, 1673, directed to Mr. COLLINS.

SIR,

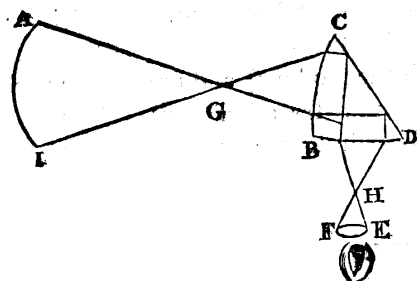
Cambridge,
April 9, 1763.

HAVING perused Mr. *Gregory's* candid reply, I have thought good to send these further considerations upon the differences that still are between us. And, first, that a well polished plate reflects, at the obliquity of 45 degrees, more truly than direct ones, seems to me very certain. For the flat *tubercula*, or shallow vallies, such as may be the remains of scratches almost worn out, will cause the least errors in the oblique rays, which fall on all sides the hill, excepting on the middle of the fore-side and back-side of it; that is, where the hill inclines directly towards or directly from the ray. For if the ray fall on that section of the hill, its error is, in all obliquities, just double of the hill's declivity. But if it fall on any other part of the hill, its error is less than double, if it be an oblique ray; and that so much the less, by how much the ray is obliquier. But if it be a direct ray, its error is just double to the declivity, and therefore greatest in that case. I presume, Mr. *Gregory*, if you think it convenient to transmit this to him, will easily apprehend me.

How the charge may be varied at pleasure in my telescope, will appear by this figure, where A represents the great concave; E, the eye-glass; and BCD, a prism of glass or crystal; whose sides, BC and BD, are not flat, but spherically convex. So that the rays which come from G, the focus of the great concave A, may, by the refraction of the first side BC, be reduced into parallelism; and after reflexion from the base, CD, be made, by the

Advantage of
oblique
Reflection
proved.

To vary the
charge at
pleasure in the
Newtonian
construction.

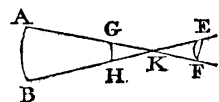


Limitation of
the aperture
of the Eye-
glass.

refraction of the next side, BD, to converge to the focus of the eye-glass H. The telescope being thus formed, it appears how the charge may be altered, by varying the distances of the glasses and speculum.

As for the objection, that Mr. Gregory's telescope will

be either overcharged or have too small an angle of vision, &c. I apprehend, that the difference between us lies in limiting the aperture of the eye-glasses. Mr. Gregory puts it equal to that of the little concave; but I should rather determine it by this proportion: That if a middle point be taken between the eye-glass and its focus; the apertures of the eye-glass and concave be proportional to their distances from that point. That is, suppose AB the little concave; EF, the eye-glass; GH, their common focus or image; and K, the mean distance between GH and EF. From the extremity of AB draw AK, and BK, cutting on the eye-glass at F and E; and EF shall be its aperture.



Reason of the
Limitation.

The reason of this limitation is, that the superfluous light, which comes on all sides of the speculum, AB, to the space, GH, in which the picture of the object is made, may fall besides the eye-glass. For if it should pass through it to the eye, it would exceedingly blend those parts of the picture with which it is mixed; and such are those parts of it which extend themselves beyond the lines AK, BK: as I remember I said in my former letter; that the scattering light, which falls on the eye-glass, will disturb the vision. And this is to be understood of any straggling light, which comes not from the picture. But if it come from the picture to the eye-glass, the disturbance will be much greater, so as not to be allowed of. Against the first I see no very convenient remedy; and against the last, none but assigning a small aperture to the eye-glass, supposing the telescope is used in the day-time, or in too great light, or to view the moon, or any star

star very near her, or near the brighter planets. And if for this reason the aperture be limited by any rule; the angle of vision will become very small, as I affirmed: for instance, in that case when Mr. Gregory in his postscript puts it about 20 degrees, it will be reduced to less than half a degree. Yet I confess there is a way, by which the angle of vision may be something enlarged, but it will not be very considerably, unless the eye-glass be also deeper charged.

Why I assign a concave with an eye-glass to magnify small objects (in Transf. p. 3080) and yet an eye-glass without such a concave to magnify the image of the great concave, which is equivalent to a small object, is, because that image doth not require to be magnified so much as an object by a microscope: and, further, because the angle of the pencil of rays, which flow from any point of the small object, that the object may appear sufficiently luminous, ought to be as great as possible: and a concave will with equal distinctness reflect the rays at a greater angle of the pencil than in a lens: but in the telescope the angles of these pencils are not so great as to transcend the limits, at which an eye-glass may, with sufficient distinctness, refract them; and therefore in these instruments, I choose to lay all the stress of magnifying upon the eye-glass as it is well capable of; and the excess only upon the concave.

Why the Eye-
glass is used
without a
Concave.

Concerning my citation of Mr. Gregory against Mr. Cassgrain, the force of it lies only in the inference, that optic instruments most probably, according to Mr. Cassgrain's design, had been tried by reflexion; which I think I might well infer, without having regard to the specific figure of the speculum, which Mr. Gregory there spoke of. And therefore I think it cannot be said, that I made him speak of spheric figures, where his meaning was of hyperbolic or elliptic ones. But if I should be so understood, because I put the figure of the great concave to be spherical, wherever I specified it; I know not why I might not, by way of consequence, make that interpretation. For it is not probable, that any man would attempt hyperbolic and elliptic figures of speculums, until the event of spherical ones had been first tried.

And accordingly the trial of Mr. *Gregory* with Mr. *Reeve* was by a spherical figure; which trial, although I am now satisfied that it was made very rudely, yet by the information that I had of it, when I wrote the letter about Mr. *Cassegrain*, I apprehended it to have been made with great diligence and curiosity; as I signified in my former letter at large. And this I hope may excuse me for speaking of it in the Transactions, as if it had been tried with more accuracy than really it was. And thus much concerning the telescope.

The design of the burning speculum appears to me very plausible, and worthy of being put in practice. What artists may think of it, I know not; but the greatest difficulty in the practice, that occurs to me, is to proportion the two surfaces so, that the force of both may be in the same point according to the theory; but perhaps it is not necessary to be so curious. For it seems to me, that the effect would scarce be sensibly less, if both sides should be ground to the concave and gage of the same tool.

L E T T E R S

RELATING TO THE

THEORY OF LIGHT AND COLOURS

LETTERS

To Mr. OLDENBURG.

SIR,

Cambridge,
Feb. , 1673.

TO perform my late promise to you, I shall without further ceremony acquaint you, that in the year 1666 (at which time I applied myself to the grinding of optick-glasses of other figures than spherical) I procured me a triangular glass-prism, to try therewith the celebrated phenomena of colours. And in order thereto, having darkened my chamber, and made a small hole in my window-shuts, to let in a convenient quantity of the sun's light, I placed my prism at its entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing divertisement, to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I became surpris'd to see them in an oblong form; which, according to the received laws of refractions, I expected should have been circular. They were terminated at the sides with streight lines, but at the ends the decay of light was so gradual, that it was difficult to determine justly what was their figure, yet they seem'd semicircular.

Comparing the length of this coloured *spectrum* with its breadth, I found it about five times greater; a disproportion so extravagant, that it excited me to a more than ordinary curiosity of examining from whence it might proceed. I could scarce think, that the various thickness of the glass, or the termination with

Of the
coloured
prismatic
Spectrum.

Proportion of
the length of
the Spectrum
to its breadth.

The disproportion no effect of the confine of the shade.

with shadow or darkness, could have any influence on light to produce such an effect: yet I thought it not amiss, first to examine those circumstances, and so tried what would happen by transmitting light through parts of the glass of divers thickneses, or through holes in the window of divers bignesses, or by setting the prism without, so that the light might pass through it, and be refracted, before it was terminated by the hole: but I found none of those circumstances material. The fashion of the colours was in all these cases the same.

Nor of any unevenness in the glass.

Then I suspected, whether by any unevenness in the glass, or other contingent irregularity, these colours might be thus dilated. And to try this, I took another prism like the former, and so placed it, that the light passing through them both, might be refracted contrary ways, and so by the latter returned into that course from which the former had diverted it: for by this means I thought the regular effects of the first prism would be destroyed by the second prism, but the irregular ones more augmented, by the multiplicity of refractions. The event was, that the light, which by the first prism was diffused into an oblong form, was by the second reduced into an orbicular one, with as much regularity as when it did not at all pass through them. So that whatever was the cause of that length, it was not any contingent irregularity.

Nor of the difference of incidence of rays from different parts of the Sun.

I then proceeded to examine more critically, what might be effected by the difference of the incidence of rays coming from divers parts of the sun; and to that end, measured the several lines and angles belonging to the image. Its distance from the hole or prism was 22 foot; its utmost length $13\frac{1}{4}$ inches; its breadth $2\frac{1}{8}$; the diameter of the hole $\frac{1}{4}$ of an inch. The angle which the rays, tending towards the middle of the image, made with those lines, in which they would have proceeded without refraction, was 44 deg. 56 min. and the vertical angle of the prism 63 deg. 12 min. Also the refractions on both sides the prism, that is, of the incident and emergent rays, were, as near as I could make them, equal; and consequently about 54 deg. 4 min. And the rays fell perpendicularly upon the wall. Now subtracting the diameter of the hole from the length and breadth

of the image, there remains 13 inches in the length, and $2\frac{1}{8}$ the breadth, comprehended by those rays which passed through the center of the said hole; and consequently the angle of the hole, which that breadth subtended, was about 31 min. answerable to the sun's diameter; but the angle which its length subtended, was more than 5 such diameters, namely, 2 deg. 49 min.

Having made these observations, I first computed from them the refractive power of that glass, and found it measured by the ratio of the sines 20 to 31; and then by that ratio I computed the refractions of two rays flowing from opposite parts of the sun's *discus*, so as to differ 31 min. in their obliquity of incidence, and found that the emergent rays should have comprehended an angle of about 31 min. as they did before they were incident.

But because this computation was founded on the hypothesis of the proportionality of the sines of incidence and refraction, which though by my own experience I could not imagine to be so erroneous, as to make that angle but 31 min. which in reality was 2 deg. 49 min. yet my curiosity caused me again to take my prism: and having placed it at my window, as before, I observed, that by turning it a little about its axis to and fro, so as to vary its obliquity to the light, more than an angle of 4 or 5 degrees, the colours were not thereby sensibly translated from their place on the wall; and consequently by that variation of incidence, the quantity of refraction was not sensibly varied. By this experiment, therefore, as well as by the former computation, it was evident, that the difference of the incidence of rays, flowing from divers parts of the sun, could not make them after decussation diverge at a sensibly greater angle, than that at which they before converged; which being at most but about 31 or 32 min. there still remained some other cause to be found out, from whence it could be 2 deg. 49 min.

Then I began to suspect, whether the rays, after their trajectory through the prism, did not move in curve lines, and according to their more or less curvity, tend to divers parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis-ball, struck with an oblique racket, describe such a curve line. For, a circular as well as a progressive motion

Nor from a curvilinear motion of the refracted Rays.

tion being communicated to it by that stroke, its parts, on that side where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctance and re-action of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion; they ought to feel the greater resistance from the ambient æther, on that side where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it, I could observe no such curvity in them. And besides (which was enough for my purpose) I observed, that the difference betwixt the length of the image and the diameter of the hole, through which the light was transmitted, was proportionable to their distance.

Experimentum Crucis.

The gradual removal of these suspicions at length led me to the *experimentum crucis*, which was this. I took two boards, and placed one of them close *behind the prism at the window*, so that the light might pass through a small hole, made in it for the purpose, and fall on the other board, which I placed at about 12 feet distance, having first made a small hole in it also for some of that incident light to pass through. Then I placed another prism behind this second board, so that the light trajected through both the boards might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hand, and turned it to and fro slowly about its axis, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them. And I saw, by the variation of those places, that the light, tending to that end of the image towards which the refraction of the first prism was made, did in the second prism suffer a refraction considerably greater than the light tending to the other end. And so the true cause of the length of that image was detected to be no other, than that *light is not similar or homogeneous, but consists of difform rays, some of which are more refrangible than others*; so that without any difference in their

their incidence on the same medium, some shall be more *refracted* than others; and therefore that, according to their *particular degrees of refrangibility*, they were transmitted through the prism to divers parts of the opposite wall.

When I understood this, I left off my afore said glass-works; for I saw, that the perfection of telescopes was hitherto limited, not so much for want of glasses truly figured according to the prescriptions of optick authors (which all men have hitherto imagined) as because that *light* itself is an heterogeneous mixture of *differently refrangible rays*: so that were a glass so exactly figured as to collect any one sort of rays into one point, it could not collect those also into the same point, which, having the same incidence upon the same medium, are apt to suffer a different refraction. Nay, I wondered, that seeing the difference of refrangibility was so great as I found, telescopes should arrive to that perfection they are now at: for, measuring the refractions in one of my prisms, I found, that supposing the common sine of incidence upon one of its planes was 44 parts, the sine of refraction of the utmost rays on the red end of the colours, made out of the glass into the air, would be 68 parts; and the sine of refraction of the utmost rays on the other end, 69 parts; so that the difference is about a 24th or 25th part of the whole refraction. And, consequently, the object-glass of any telescope cannot collect all the rays which come from one point of an object, so as to make them convene at its focus in less room than in a circular space, whose diameter is the 50th part of the diameter of its aperture; which is an irregularity, some hundreds of times greater than a circularly figured *lens*, of so small a section as the object-glasses of long telescopes are, would cause by the unsuitness of its figure, were light uniform.

This made me take reflections into consideration; and finding them regular, so that the angle of reflection of all sorts of rays was equal to their angle of incidence, I understood, that by their mediation optick instruments might be brought to any degree of perfection imaginable, provided a reflecting substance could be found which would polish as finely as glass, and reflect as much light as glass transmits, and the art of communicating to it a parabolick

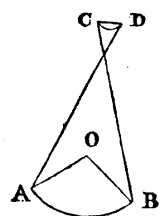
rabolick figure be also attained. But these seemed very great difficulties, and I have also thought them insuperable, when I farther considered, that every irregularity in a reflecting superficies makes the rays stray 5 or 6 times more out of their due course, than the like irregularities in a refracting one: so that a much greater curiosity would be here requisite, than in figuring glasses for refraction.

First
Reflecting
Telescope.

Amidst these thoughts, I was forced from *Cambridge*, anno 1666, by the intervening plague, and it was more than two years before I proceeded further. But then having thought on a tender way of polishing, proper for metal, whereby, as I imagined, the figure would be corrected to the last; I began to try what might be effected in this kind, and by degrees so far perfected an instrument, in the essential parts of it like that I sent to *London*, by which I could discern *Jupiter's* four concomitants, and shewed them divers times to two others of my acquaintance. I could also discern the moon-like *phase* of *Venus*, but not very distinctly, nor without some niceness in disposing the instrument.

From that time I was interrupted till this last autumn, when I made another. And as that was sensibly better than the first, especially for day-objects, so I doubt not but they will be still brought to a much greater perfection by their endeavours, who, as you inform me, are taking care about it at *London*.

I have sometimes thought to make a microscope, which should have, instead of an object-glass, a reflecting piece of metal. For



these instruments seem as capable of improvement as telescopes, and perhaps more; because but one reflective piece of metal is requisite in them, as you may perceive by the diagram; where AC representeth the object-metal; CD, the eye-glass; F, their common focus; and O, the other focus of the metal, in which the object is placed.

But to return from this digression, I told you, that light is not similar or homogenous, but consists of difform rays, some of which are more refrangible than others: so that of those which are alike incident on the same medium, some shall be more refracted than others; and that not by virtue of the glass, or other external

external cause, but from a pre-disposition, which every particular ray hath to suffer refraction.

I SHALL now proceed to acquaint you with another more notable *Origin of* difformity in its rays, wherein the origin of colours is unfolded: concerning which I shall lay down the doctrine first; and then, for its examination, give you an instance of two of the experiments, as a specimen of the rest.

The doctrine you will find comprehended and illustrated in the following propositions.

I. As the rays of light differ in degrees of refrangibility, so they also differ in their disposition to exhibit this or that particular colour. Colours are not qualifications of light, derived from refractions, or reflections of natural bodies (as it is generally believed) but original and connate properties, which in divers rays are divers. Some rays are disposed to exhibit a Red colour, and no other; some a Yellow, and no other; some a Green, and no other; and so of the rest. Nor are there only rays proper and particular to the more eminent colours, but even to all their intermediate gradations.

II. To the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility. The least refrangible rays are all disposed to exhibit a Red colour; and contrarily, those rays which are disposed to exhibit a Red colour, are all the least refrangible: so the most refrangible rays are all disposed to exhibit a deep Violet colour; and contrarily, those which are apt to exhibit such a Violet colour, are all the most refrangible: and so to all the intermediate colours in a continued series belong intermediate degrees of refrangibility. And this analogy betwixt colours and refrangibility is very precise and strict; the rays always either exactly agreeing in both, or proportionably disagreeing in both.

III. The species of colour, and degree of refrangibility proper to any particular sort of rays, is not mutably by refraction, nor by reflection from natural bodies, nor by any other cause that I could yet observe. When any one sort of rays hath been well parted from those of other kinds, it hath afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change.

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change it. I have refracted it with prisms, and reflected it with bodies, which in day-light were of other colours; I have intercepted it with the coloured film of air, interceding two compressed plates of glass; transmitted it through coloured mediums, and through mediums irradiated with other sorts of rays, and diversly terminated it; and yet could never produce any new colour out of it. It would, by contracting or dilating, become more brisk, or faint, and by the loss of many rays, in some cases very obscure and dark; but I could never see it changed in specie.

IV. Yet seeming transmutations of colours may be made, where there is any mixture of divers sorts of rays: for in such mixtures, the component colours appear not; but, by their mutual allaying each other, constitute a middling colour. And therefore, if by refraction, or any other of the aforefaid causes, the difform rays, latent in such a mixture, be separated, there shall emerge colours different from the colour of the composition. Which colours are not new generated, but only made apparent by being parted; for if they be again intirely mixed and blended together, they will again compose that colour, which they did before separation. And, for the same reason, transmutations, made by the convening of divers colours, are not real; for when the difform rays are again severed, they will exhibit the very same colours, which they did before they entered the composition; as you see Blue and Yellow powders, when finely mixed, appear to the eye Green; and yet the colours of the component corpuscles are not thereby really transmuted, but only blended. For when viewed with a good microscope, they still appear Blue and Yellow interperfectly.

V. There are therefore two sorts of colours, the one original and simple, the other compounded of these. The original or primary colours are, Red, Yellow, Green, Blue, and a Violet-Purple, together with Orange, Indico, and an indefinite variety of intermediate gradations.

VI. The same colours in specie, with these primary ones, may be also produced by composition. For a mixture of Yellow and Blue makes Green; of Red and Yellow makes Orange; of Orange and Yellowish Green makes Yellow. And, in general, if

if any two colours be mixed, which in the series of those gene-Colours. rated by the prism are not too far distant from one another; they, by their mutual alloy, compound that colour, which in the said series appeareth in the midway between them. But those which are situated at too great a distance, do not so. Orange and Indico produce not the intermediate Green, nor Scarlet and Green the intermediate Yellow.

VII. But the most surprizing and wonderful composition was that of Whiteness. There is no one sort of rays which alone can exhibit this. It is ever compounded; and to its composition are requisite all the aforefaid primary colours, mixed in a due proportion. I have often with admiration beheld, that all the colours of the prism being made to converge, and thereby to be again mixed, as they were in the light before it was incident upon the prism, reproduced light entirely and perfectly White, and not at all sensibly differing from a direct light of the sun, unless when the glasses I used were not sufficiently clear; for then they would a little incline it to their colour.

VIII. Hence, therefore, it comes to pass, that Whiteness is the usual colour of light; for light is a confused aggregate of rays: indued with all sorts of colours, as they were promiscuously darted from the various parts of luminous bodies. And of such a confused aggregate, as I said, is generated Whiteness, if there be a due proportion of the ingredients; but if any one predominate, the light must incline to that colour; as it happens in the blue flame of brimstone; the yellow flame of a candle; and the various colours of the fixed stars.

IX. These things considered, the manner how colours are produced by the prism is evident. For, of the rays, constituting the incident light, since those which differ in colour proportionally differ in refrangibility, they, by their unequal refractions, must be severed and dispersed into an oblong form in an orderly succession, from the least refracted Scarlet, to the most refracted Violet. And for the same reason it is, that objects, when looked upon through a prism, appear coloured. For the difform rays, by their unequal refractions, are made to diverge towards several parts of the *retina*, and there express the images of things coloured;

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loured; as in the former case they did the sun's image upon a wall. And by this inequality of refractions, they become not only coloured, but also very confused and indistinct.

X. Why the colours of the rainbow appear in falling drops of rain, is also from hence evident. For those drops, which refract the rays disposed to appear Purple in greatest quantity to the spectator's eye, refract the rays of other sorts so much less, as to make them pass beside it; and such are the drops on the inside of the primary bow, and on the outside of the secondary or exterior one. So those drops, which refract in greatest plenty the rays apt to appear Red toward the spectator's eye, refract those of other sorts so much more, as to make them pass beside it; and such are the drops on the exterior part of the primary, and interior part of the secondary bow.

XI. The odd phenomena of an infusion of *lignum nepbriticum*, leaf-gold, fragments of coloured glass, and some other transparently coloured bodies, appearing in one position of one colour, and of another in another, are on these grounds no longer riddles. For those are substances apt to reflect one sort of light, and transmit another; as may be seen in a dark room, by illuminating them with similar or uncompound light. For then they appear of that colour only with which they are illuminated; but yet in one position more vivid and luminous than in another, accordingly as they are disposed more or less to reflect or transmit the incident colour.

XII. From hence also is manifest the reason of an unexpected experiment, which Mr. Hook, somewhere in his *Micrography*, relates to have made with two wedge-like transparent vessels, filled the one with a Red, the other with a Blue liquor: namely, that though they were severally transparent enough, yet both together became opaque: for if one transmitted only Red, and the other only Blue, no rays could pass through both.

XIII. I might add more instances of this nature, but I shall conclude with this general one: That the colours of all natural bodies have no other origin than this; that they are variously qualified to reflect one sort of light in greater plenty than another. And this I have experienced in a dark room, by illuminating those

those bodies with uncompound light of divers colours. For ^{Colour.} by that means any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light cast upon them; but yet with this difference, that they are most brisk and vivid in the light of their own daylight colour. *Minium* appeareth there of any colour indifferently, with which it is illustrated; but yet most luminous in Red: and so Bise appeareth indifferently of any colour, with which it is illustrated; but yet most luminous in Blue: and therefore *Minium* reflecteth rays of any colour, but most copiously those endowed with Red; and consequently when illustrated with day-light, that is, with all sorts of rays promiscuously blended, those qualified with Red shall abound most in the reflected light, and by their prevalence cause it to appear of that colour. And for the same reason Bise, reflecting Blue most copiously, shall appear Blue by the excess of those rays in its reflected light; and the like of other bodies. And that this is the entire and adequate cause of their colours is manifest, because they have no power to change or alter the colours of any sort of rays incident apart, but put on all colours indifferently, with which they are enlightened.

These things being so, it can be no longer disputed, whether ^{Light not a Quality.} there be colours in the dark, or whether they be the qualities of the objects we see; no, nor perhaps whether light be a body. For, since colours are the qualities of light, having its rays for their intire and immediate subject, how can we think those rays qualities also, unless one quality may be the subject of, and sustain another? which, in effect, is to call it substance. We should not know bodies for substances, were it not for their sensible qualities; and the principal of those being now found due to something else, we have as good reason to believe that to be a substance also.

Besides, who ever thought any Quality to be a heterogeneous aggregate, such as light is discovered to be? But to determine more absolutely what light is, after what manner refracted, and by what modes or actions it produceth in our minds the phantasms of colours, is not so easy: and I shall not mingle conjectures with certainties.

REVIEWING what I have written, I see the discourse itself will lead to divers experiments sufficient for its examination: and therefore I shall not trouble you farther than to describe one of those which I have already insinuated.

Proof by
Experiment.

In a darkened room make a hole in the shut of a window, whose diameter may conveniently be about a third part of an inch, to admit a convenient quantity of the sun's light: and there place a clear and colourless prism, to refract the entering light towards the further part of the room; which, as I said, will thereby be diffused into an oblong coloured image. Then place a lens of about 3 foot radius, suppose a broad object-glass of a three foot telescope, at the distance of about 4 or 5 foot from thence, through which all those colours may at once be transmitted, and made by its refraction to convene at a further distance of about 10 or 12 feet. If at that distance you intercept this light with a sheet of white paper, you will see the colours converted into Whiteness again by being mingled. But it is requisite that the prism and lens be placed steady, and that the paper, on which the colours are cast, be moved to and fro; for by such motion you will not only find at what distance the Whiteness is most perfect, but also see how the colours gradually convene and vanish into whiteness; and afterwards, having crossed one another in that place where they compound whiteness, are again diffipated and severed, and, in an inverted order, retain the same colours which they had before they entered the composition. You may also see, that if any of the colours at the lens be intercepted, the whiteness will be changed into the other colours. And therefore, that the composition of whiteness be perfect, care must be taken that none of the colours fall besides the lens. In the annexed design of this experiment, ABC [*vid. Lect. Opt. Par. II. Tab. III. fig. 14.*] expresseth the prism set endwise to light, close by the hole F, of the window EG. Its vertical angle, ACB, may conveniently be about 63 deg. MN designeth the lens: its breadth, $2\frac{1}{2}$, or 3 inches. OF, one of the straight lines, in which difform rays may be conceived to flow successively from the sun. FP, and FT, two of those rays unequally refracted, which the lens makes to converge towards x, and after decussation to diverge

verge again. And FT, x, π , the paper, at divers distances, on which the colours are projected; which in x constitute whiteness, but are Red and Yellow in τ , and τ , and Blue and Purple in p, and π .

If you proceed further to try the impossibility of changing any uncompound colour, which I have asserted in the 3d and 13th Propositions, it is requisite that the room be made very dark; lest any scattering light, mixing with the colour, disturb and allay it, and render it compound contrary to the design of the experiment: it is also requisite that there be a perfecter separation of the colours, than, after the manner above described, can be made by the refraction of one single prism; and how to make such further separations, will scarce be difficult to them that consider the discovered laws of refractions. But if trials shall be made with colours not thoroughly separated, there must be allowed changes proportionable to the mixture. Thus if compound Yellow light fall upon the Blue bise, the bise will not appear perfectly Yellow, but rather Green: because there are in the Yellow mixture many rays endued with Green; and Green, being less remote from the usual Blue colour of bise than Yellow, is the more copiously reflected by it.

In like manner, if any one of the prismatic colours, suppose Red, be intercepted, on design to try the asserted impossibility of reproducing the colour out of the others which are pretermitted; it is necessary, either that the colours be very well parted before the Red be intercepted, or that together with the Red, the neighbouring colours, into which any Red is secretly dispersed, that is, the Yellow and perhaps Green too, be intercepted: or else, that allowance be made for the emerging of so much Red out of the Yellow-green, as may possibly have been diffused, and scatteringly blended in those colours. And if these things be observed, the new production of Red, or of any other intercepted colour, will be found impossible.

This I conceive is enough for introduction to experiments of this kind; which, if any of the Royal Society shall be so curious as to prosecute, I shall be very glad to be informed with what success: that if any thing seem to be defective, or to thwart this relation,

I may have an opportunity of giving further direction about it; or of acknowledging my errors, if I have committed any.

II.

NEWTONUS OLDENBURGO S.

Reply to
Ignatius
Pardies.

ACCEPI observationes Reverendi Patris Ignatii Pardies in Epistolam meam de Lucis refractionibus et Coloribus ad te conscriptam; quo nomine me illi devinctum agnosco; atque hæc difficultatibus, quas proposuit, diluendis rescribo.

The length of the prismatic image of the Sun not caused by unequal incidence of the Rays. Imprimis ait longitudinem solaris imaginis, à refractione prismatis refractione effectam, non aliâ indigere causâ, quàm diversâ radiorum ab oppositis partibus solaris disci profluentium incidentia; adeoque non probare diversam refrangibilitatem radiorum. Et quo assertionis ejus veritatem confirmet, ostendit casum, in quo ex diversâ incidentiâ triginta minutorum, differentia refractionis potest esse duorum graduum cum viginti tribus minutis, vel etiam paulo major, prout exigit meum experimentum. Sed hallucinatus est reverendus Pater: nam refractiones à diversâ parte prismatis, quantum potest inæquales statuit R. P. Pardies, cum tamen ego tum in experimentis, tum in calculo de experimentis istis inito, æquales adhibuerim. Sit autem ABC * prismatis sectio ad axem ejus perpendicularis; XL, & YK, radii duo in F (medio foraminis) decussantes, & in prisma illud incidentes ad K, & L; sintque eorum refracti KH, & LI, ac denuo HP, & IT. Et cum refractiones ad latus AC, æquales esse refractionibus ad latus BC, quàm proximè supposuerim; si AC, & BC, statuatur æqualia, similis erit radiorum KH, & LI ad AB, basin prismatis, inclinatio; adeoque ang. CLI = ang. CHK; & ang. CIL = ang. CKH. Quare etiam refractiones in K, & I, æquales erunt; ut & in L, & H; atque adeo ang. PHA = ang. FLB; & ang. TIA = ang. BKF; & proinde refractorum HP, & IT, eadem erit ad invicem inclinatio, ac est incidentium radiorum FL, & FK. Sit ergo angulus KFL, 30 min. æqualis nempe solari diametro, & erit angulus, quem HI, & mn, comprehendunt, etiam 30 min. si modo radii FL, & FK, æqualiter refrangibiles statuatur. At mihi experimenti prodiit angulus ille circiter 2 gr. 49 min. quem radius HP,

* Vide Opt. Book I. Par. I. Tab. III. fig. 13.

extremum

extremum violaceum colorem, & mn, cæruleum exhibens, constitutur; ac proinde radios illos diversimode refrangibiles esse, sive refractiones secundum disparem finuum incidentiæ & refractionis rationem peragi, necessario concedendum est.

Addit præterea R. P. quòd non sufficit ad obeundum ritè calculum, ex longitudine imaginis impactæ in chartam subtrahere magnitudinem foraminis fenestræ; quandoquidem etiam posito foramine indivisibili, adhuc fieret aliud veluti foramen latum in posteriori superficie prismatis. Mihi tamen videtur, his non obstantibus, quòd refractiones radiorum, in anteriori æquè ac posteriori superficie prismatis decussantium, ex adhibitis principiis possint ritè computari. Sed si res secus esset, latitudo hiatus in posteriori superficie, quod ad instar foraminis est, haud efficeret errorem duorum minutorum secundorum; & in rebus practicis non operæ pretium duco ad minutias istas attendere.

Illi insuper experimento, quod *Crucis* vocaveram, nihil adversatur R. P. dum contendit, inæquales radiorum, diversis coloribus imbutorum, refractiones, ex inæqualibus incidentiis effectas fuisse. Nam radiis per duo admodum parva, ab invicem distantia, & immota foramina transeuntibus, incidentiæ illæ, prout ego experimentum institui, omnino æquales erant, & tamen refractiones liquido inæquales. Sin ille de experimentis nostris dubitet, oro, ut radiorum diversis coloribus præditorum refractiones ex incidentiis paribus mensuret, & sentiet inæquales esse. Si modus ille, quem ego ad hoc negotium adhibui, minùs placeat (quo tamen nullus potest esse luculentior) facile est alios excogitare, sicut & alios ipse haud paucos cum fructu expertus sum.

Contra Theoriam de coloribus objicitur, quòd pulveres diversorum colorum permixti non candidum sed subobscurum & fuscum colorem exhibent. Mihi vero albus, niger & omnes intermedii fusci, qui ab albo & nigro permixtis componi possunt, non specie coloris sed quantitate lucis tantum differre videntur. Et cum in mixtione pigmentorum, singula corpuscula non nisi proprium colorem reflectant, adeoque maxima pars lucis incidentis supprimatur & retineatur: lux reflexa subobscura evadet, & quasi cum tenebris permixta; adeo ut non intensum alborem, sed qualem nigredinis permixtio conficit, hoc est fuscum, exhibere debeat.

Objicitur

Objection
from the
mixture of
coloured
fluids in the
same vessel
answered.

Obijcitur deinde, quod à liquoribus quibuscunque diverſi coloris in eodem vaſe commiſtis, æquè ac in diverſis vaſis contentis, opacitas oriri debet; quod tamen, ait, verum non eſſe. Sed non video conſequentiam. Nam plurimi liquores agunt in ſe invicem, & novam ſibi mutuò partium contexturam ſecreto inducunt; unde opaci, diaphani, vel variis coloribus, ex coloribus permiſtorum nullo modo oriundis, præditi evadere poſſunt. Et hæc de cauſa experimenta hujusmodi minùs apta ſemper exiſtimavi, à quibus concluſiones deduci poſſint. Subnoto tamen, quod ad hoc experimentum requiruntur liquores ſaturis & intenſis coloribus præditi, qui perpaucos niſi proprii coloris radios transmittant; quales rarò occurrunt, ut videbitur illuminando liquores cum diverſis coloribus priſmatis in obſcurato cubiculo. Nam pauci reperiuntur, qui in propriis coloribus ſatis diaphani appareant, inque alienis opaci. Convenit præterea, ut adhibiti colores ſint inter ſe oppoſiti, quales exiſtimo fore rubrum & cæruleum, vel flavum & violaceum, vel etiam viridem & purpureum illum qui coccineo affinis eſt. Et ex hujusmodi liquoribus nonnulli, quorum partes tingentes non congregiuntur, fortasſe permiſti evadunt opaciores. Sed de eventu nihil ſum ſollicitus, tum quod luculentius eſt experimentum in liquoribus ſeorſim exiſtentibus, tum quod experimentum illud, ſicut & iridis, tincturæ nephriticæ, & aliorum corporum naturalium phænomena, non ad probandam ſed ad illuſtrandam tantum doctrinam propoſui.

Quod R.P. Theoriam noſtram *hypothefin* vocat, amicè habeo, ſiquidem ipſi nondum conſtet. Sed alio tamen conſilio propoſueram, & nihil aliud continere videtur quàm proprietates quædam lucis, quas jam inventas probare haud difficile exiſtimo, & quas ſi non veras eſſe cognoveram, pro futili & inani ſpeculatione malletm repudiare, quàm pro meâ *hypothefi* agnoſcere.

Quid verò cenſeri mereatur, ex reſponſionibus ad animadverſiones Domini——fortasſe ſtatim prodituris clariùs patebit. Interim vale, et perge amare tibi devinctiſſimum

III.

III.

To Mr. O L D E N B U R G .

S I R ,

Cambridge,
April , 1672.

I herewith * ſend you an answer to the Jeſuit *Pardie's* confiderations, in the concluſion of which, you may poſſibly apprehend me a little too poſitive; but I ſpeak only for myſelf. I am highly ſenſible of your good will, in communicating to me ſuch obſervations as occur concerning my theories, or cata-dioptrical inſtruments, and I deſire you to continue that favour to me. Mr. *Hugens* has very well obſerved the confuſion of refractions near the edges of a lens, where its two ſuperficies are inclined much like the planes of a priſm; whoſe refractions are in like manner confuſed. But it is not from the inclination of thoſe ſuperficies, ſo much as from the heterogeneity of light, that that confuſion is cauſed. For by illuminating an object with homogeneous light, I have ſeen it far diſtinctor through a priſm, than I could by light that was heterogeneous.

I ſuppoſe the deſign of Sir *Robert Moray's* experiments is, to have their events expreſſed with ſuch obſervations as may occur concerning them.

Touching the firſt, I have obſerved, that the ſolar image falling on a paper, placed at the focus of the lens, was, by the interpoſed priſm, drawn out in length proportional to the priſm's refraction, or diſtance from that focus. And the chief obſervable here, which I remember, was, that the ſtreight edges of the oblong image were diſtinctor than they would have been without the lens.

Conſidering that the rays, coming from the planet *Venus*, are much leſs inclined one to another, than thoſe which come from the oppoſite parts of the ſun's diſk; I once tried an experiment or two with her light. And to make it ſufficiently ſtrong, I found it neceſſary to collect it firſt by a broad lens; and then, interpoſing a priſm between the lens and its focus, at ſuch diſtance

* This encloded the preceding Letter.

that

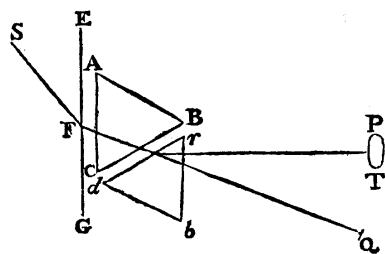
that all the light might pass through the prism, I found the focus, which before appeared like a lucid point, to be drawn out into a long splendid line by the prism's refraction.

I have sometimes designed to try how a fixed star, seen through a long telescope, would appear, by interposing a prism between the telescope and my eye. But by the appearance of *Venus*, viewed with my naked eye through a prism, I preface the event.

Concerning the second experiment, I have occasionally observed, that by covering both ends of the prism with paper at several distances from the middle, the breadth of the solar image will be increased or diminished as much, as is the aperture of the prism, without any variation of the length: or, if the aperture be augmented on all sides, the image on all sides will be so much and no more augmented.

Of the third experiment, I have occasion to speak in my answer to another person; where you will find the effects of two prisms, in all cross positions of one to another, described. But if one prism alone be turned about, the coloured image will only be translated from place to place, describing a circle, or some other conick section on the wall, on which it is projected, without suffering any alteration in its shape, unless such as may arise from the obliquity of the wall, or casual change of the prism's obliquity to the sun's rays.

The effect of the fourth experiment I have already insinuated, telling you, that light, passing through parts of the prism of divers thicknesses, did still exhibit the same phenomena.



in it through which the light arrives at the prisms; ABC, the first prism, which refracts the light towards PT, painting there the colours

colours in an oblong; and *abr* the second prism, which refracts back again the rays to Q, where the long image, PT, is contracted into a round one. I suppose the plane *abr* parallel to BC, and *br* to AC, that no rays may be equally refracted contrary ways in both prisms. The prisms must also be placed very near to one another; for if their distance be so great, that colours begin to appear in the light, before its incidence on the second prism, those colours will not be destroyed by the contrary refractions of that prism.

These things being observed, the round image, Q, will appear of the same bigness, which it doth when both the prisms are taken away, that the light may pass directly towards Q from the hole without any refraction at all: and its diameter will equal the breadth of the long image PT, if those images be equally distant from the prisms.

If an accurate consideration of these refractions be designed, it is convenient that a lens be placed in the hole F, or immediately after the prisms, so that its focus be at the image Q, or PT. For thereby the perimeter of the image Q, and the straight sides of the image PT, will become much better defined than otherwise.

Thus far concerning Sir *Robert Moray's* proposals. I have nothing more at present, unless to desire you, that, in the letter wherein I sent you of the table of apertures and charges, you would change an expression concerning the six-foot tube, where I intimated, that it was none of the best in its kind. For lest the friend, of whom it was borrowed, should think I depreciate it, I had rather that the expression should be a little intimated after this manner; that I am not very well assured of its goodness, and therefore desire, that the other experiment of reading at 100 foot distance, shall rather be confided in. You will do me a favour to peruse the rest of that letter also, before you commit it to the press. For I writ it in so much haste, that I had no time to peruse it: and by rendering my expressions more perspicuous, or less ambiguous, you will still oblige, yours, &c.

Reply to further objections of Ignatius Pardies.

IN observationibus R. P. Ignatii Pardies, quas ad te denuo conscripsit, an majus sit humanitatis argumentum, quod meis responsionibus vim omnem attribuit, an ingenii, quod objectiones proponit, quæ si non probè tollantur, doctrinam meam frustrari possunt, vix dixerim. Utrumque sanè ad determinandam veritatem optimè conducit; efficitque, ut acceptis quàm lubentissimè respondeam.

Objection. That the length of the coloured image may be explained by Grimaldi's hypothesis, or by Hook's.

Ait R. P. quod absque variâ diversorum radiorum refrangibilitate, possibile sit explicare longitudinem colorum; puta ex hypothesi *P. Grimaldi*, per diffusionem luminis, quod supponitur esse substantia quædam rapidissimè mota; vel ex hypothese *Hookii* nostri, per diffusionem vel expansionem undulationum, quas statuit in æthere à lucidis corporibus excitatas, quaquaversum propagari. Addo, quod ex hypothese *Cartesiana* potest etiam effingi consimilis diffusio conatûs vel pressionis globulorum; perinde ut in explicatione caudæ cometæ supponitur. Et eadem diffusio vel expansio juxta aliam quamvis hypothesein, in quâ lumen statuitur esse vis, actio, qualitas, vel substantia quælibet, à luminosis corporibus undique emissâ, effingi potest.

Ut his respondeam, animadvertendum est, quod doctrina illa, quam de refractione & coloribus explicui, in quibusdam lucis proprietatibus solummodo constitit, neglectis hypotheseibus per quas proprietates illæ explicari debent.

Optimus enim et tutissimus philosophandi modus videtur, ut imprimis rerum proprietatis diligenter inquiramus, & per experimenta stabiliamus; ac dein tardiùs contendamus ad hypotheseis pro rerum explicatione. Nam hypotheseis ad explicandas rerum proprietates tantùm accommodari debent, & non ad determinandas usurpari, nisi quatenus experimenta subministrare possint. Et si quis ex solâ hypotheseium possibilitate de veritate rerum conjecturam faciat, non video quo pacto quicquam certi in ullâ scientiâ determinare possit; siquidem alias atque alias hypotheseis semper licet excogitari, quæ novas difficultates suppeditare videbuntur.

Quamobrem

Quamobrem ab hypotheseium contemplatione, tanquam improprio argumentandi loco, hic abstinendum esse censeo, & vim objectionis abstrahendam, ut pleniorè & magis generalem responsionem accipiat.

Itaque per lumen intelligo quodlibet ens vel entis potestatem (sive sit substantia, sive quævis ejus vis, actio, vel qualitas) quod à corpore lucido rectâ pergens aptum sit ad excitandam visionem; & per radios luminis intelligo minimas vel quælibet indefinitè parvas ejus partes, quæ ab invicem non dependent, quales sunt illi omnes radii, quos lucentia corpora, vel simul vel successivè, secundum rectas lineas emittunt. Nam illæ tum collaterales tum successivæ partes luminis sunt independentes; siquidem unæ absque aliis intercipi possint, & in quælibet plagas seorsim reflecti vel refringi. Et hoc præcognito, objectionis vis omnis in eo sita erit; quod colores per aliquam luminis ultra foramen diffusionem, quæ non oritur ab inæquali diversorum radiorum seu luminis independentium partium refrangibilitate, in longum diduci possint.

Quod autem non aliunde oblongentur superiùs monstravi, in literis relatis in Philosoph. Transact. Numb. 80. Et ut rationes faciliùs perspiciantur, non gravabor jam fusiùs explicare.

Scilicet ex observatione, quod Radii post Refractionem non incurvabantur, sed rectâ ad parietem progressi fuerint, patuit, eandem fuisse eorum ad se mutuò inclinationem cum modò exierunt prismate, atque cum impeerunt in parietem; & proinde longitudo colorum ex inclinatione radiorum emerfit, quam inter refringendum obtinuerunt; hoc est ex quantitate refractionis, quam singuli radii in prismate patiebantur. Adeoque cum colorum longitudo latitudinem aliquot vicibus ex observatione superavit; sequitur majorem fuisse inæqualitatem refractionum, quàm potuit oriri ex inæqualitate incidentiarum. Quinimo ex figurâ Imaginis coloratæ, quod nempe non fuit Ovalis, sed latera duabus parallelis rectis lineis terminata, patuit eam ex indefinitè multis imaginibus Solis, per inæqualem refractionem in longam diffractis & serie continuâ dispositis, constitui; adeoque radios, à singulis partibus solaris disci provenientes, per totam ferè longitudinem colorum dispergi; & proinde similiter incidentium inæquales esse refractiones. Id quod aliis etiam indiciis ostendi posset.

Reply. Definition of Light, and Rays of Light.

The fact proved, that Rays are unequally refracted in equal incidences.

Of the cause
of that fact.

Constat itaque diversas esse refractiones, ubi pares sunt incidentiæ. Sed amplius inquirendum est, unde oriatur illa diversitas? An sit à causâ aliquâ incertâ & irregulari, vel certâ lege, secundum quam radius quilibet aptus est determinatam aliquam refractionem pati. Per incertas & irregulares causas intellige asperitates in superficie; vel venas diversæ densitatis in interiori parte Vitri, ex quo Prisma conflatur; item irregularem situm pororum, quos nonnulli ob luminis transmissionem per vitrum omnifariam trajici statuunt; necnon tremores & inæquales commotiones partium Ætheris, Aeris vel Vitri; radorum, in refringente superficie se mutuò fortasse comprimantium, resulturam ab invicem; ejusdem cujusque radii divisionem ac dissipationem in partes divergentes, quas vel numero finitas vel indefinitè multas, in superficie aliquâ continuatim jacentes, imaginari liceat; vel quamvis aliam diffusionem et dilatationem luminis, quam possumus excogitare, non ortam ex diversâ prædispositione cujusque radii ad refractionem, in certo aliquo & constanti gradu, patiendam.

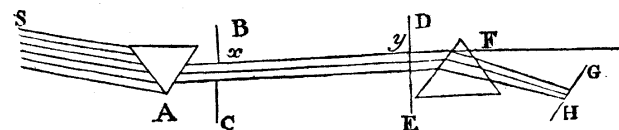
That it is no
irregular
uncertain
cause.

Quòd autem diversa refractione non orta sit ex ullis ejusmodi causis incertis & irregularibus, probavi per experimentum duorum consimilium prismatum in contrario situ juxta positorum, ita ut posterius contrariâ suâ refractione retroflecteret radios, & sic regulares effectus prioris destrueret, sed per iteratas refractiones, augeret irregulares. Utpote si prius prisma diffunderet, ac divergere faceret parallelos radios, ex. gr. per asperam polituram, inæquabilem densitatem, aut irregularem situm pororum in prismatis, vel per tremulos motus partium Ætheris, Aeris aut Vitri; vel per dilatationem luminis propter partium ejus (*i. e.* radorum) se mutuò comprimantium relaxationem versus adjacentia spatia, quæ vel nullo vel minus constipato lumine irradiantur; vel denique per cujusque radii dilatationem aut diffractionem in complures divergentes radios: tum sane posterius prisma magis diffunderet ac dissiparet radios per dictas irregularitates Ætheris, Aeris aut Vitri; vel per iteratam dilatationem luminis, à refringentis superficie resistentiâ denuò constipati ac diffusi; vel etiam per cujusque radii, à priori diffractione orti, iteratam diffractionem ac divisionem in longè plures divergentes radios. Et sic Lumen magis dispergeretur per refractionem secundi Prismatis, & in parietem projectam

projectam imaginem duplo longiorem minimum exhiberet, quàm per solam refractionem prioris Prismatis exhiberi potuisset. Quamobrem cum, experientiâ teste, refractione secundi Prismatis adeo non dispergat Lumen, ut contrahat, & in pristinum statum reducat, efficiatque ut in formâ Coni postea progrediatur, perinde ac si nullam omnino Refractionem passum fuisset; concedendum est, diffusionem Luminis, à refractione anterioris Prismatis effectam, non oriri ab aliquâ præfatarum causarum, aut aliâ quavis irregularitate, sed diversæ Refrangibilitati radorum solummodo tribuendam esse; utpote quâ radius unusquisque, ex insitâ dispositione tantam Refractionem in posteriori, ac in priori passus, reducitur in parallelismum cum se ipso; & sic omnes radii ad se mutuò easdem inclinationes resumunt, quas ante refractiones habuere.

Demum ut hæc omnia summè confirmarem, adjeci experimentum illud, quod jam nomine *crucis* passim insignitur; de cujus conditionibus cum R. P. dubitaverit, placuit jam designare Schemate. Sit BC anterior tabula, cui prisma A immediatè præfigi-

Experimentum
Crucis
described.



tur; sitque DE altera tabula, quasi 12 pedibus abinde distans, cui suffigitur alterum prisma F. Tabulæ autem ad x & y, ita prefiguntur, ut aliquantulum lucis, ab anteriori prismate refractæ, trajici possit per utrumque foramen ad secundum prisma, inque eo denuò refringi. Jam prisma anterius circa axem reciproco motu convertatur, & colores in tabulam posteriorem DE procidentes, per vices attollentur ac deprimentur; eoque pacto alius atque alius color successivè pro arbitrio trajici potest per foramen ejus y, ad posterius prisma, dum cæteri colores in tabulam impingunt: et videbis, radios diversis coloribus præditos diversam pati refractionem in illo posteriori prismate, ex eo quòd ad diversa loca parietis vel cujusvis obstaculi GH, pedibus aliquot ulterius remoti, alabantur; puta violacei radii ad H, rubri ad G, & intermedii ad loca intermedia: & tamen, propter determinatam positionem foraminum, necesse est ut similis sit incidentia radorum cujusque coloris

coloris per utrumque trajecti. Atque ita ex mensurâ constat radios, diversis coloribus affectos, habere diversas leges refractionum.

Cause of
Pardies
mistake.

Sed suspicor unde adductus sit R. P. in dubitationem; nempe videtur collocasse primum prisma, A, post tabulam BC, atque ita convertendo circa axem, verisimile est inclinationem radiorum, qui interjacent foramina, propter intermediam refractionem fuisse mutatam. At, ex descriptione prius expositâ, debuit tabula illa collocari post prisma, ut radii inter foramina in directum jacerent; quemadmodum, ex verbis, *I took two boards, and placed one of them close behind the prism at the window*, constare potest. Et usus experimenti idem innuit.

Ex abundanti placet observare, quod in hoc experimento colorata lux, ob refractionem secundi prismatis, longè minùs diffunditur ac divaricat, quàm cum alba existit, adeo ut imago ad G, vel H, sit pene circularis; præsertim si prismata statuantur parallela, & in contrario situ angulorum, prout in schemate designantur. Quinetiam, si præterea diameter foraminis y adæquet latitudinem colorum, nulla erit ejusdem coloratæ lucis in longum diffusio; sed imago, quæ à quopiam colore ad G, vel H, effingitur (positis circularibus foraminibus, & refractione posterioris prismatis non majori quàm prioris, radiisque ad obstaculum quàm proximè perpendicularibus) erit planè circularis. Id quod arguit diffusionem, de quâ suprà egimus, non ex contagione vel continuitate materiæ undulantis, aut celerrimè motæ, vel similibus causis ortam esse, sed ex certâ refractionum cujusque generis radiorum lege. Cur autem imago illa in uno casu sit circularis, & in aliis nonnihil oblongata, & quomodo diffusio lucis in longitudinem in quolibet casu pro arbitrio minui possit, à geometris determinandum, & cum experientiâ conferendum relinquo.

Postquam proprietates lucis his & similibus experimentis satis exploratæ fuerint, spectando radios tanquam ejus sive collaterales sive successivas partes, de quibus experti sumus per independentiam quod sint ab invicem distinctæ; hypotheses exinde dijudicandæ sunt, & quæ non possunt conciliari rejiciendæ. Sed levissimi negotii est accommodare hypotheses ad hanc doctrinam. Nam si quis hypothesin Cartesianam defendere velit, dicendum est, globulos esse inæquales: vel pressiones globulorum esse alias aliis fortiores,

fortiores, & inde diversimodè refrangibiles, & aptas ad excitandam sensationem diversorum colorum. Et sic, juxta hypothesin Cl. *Hookii* dicendum est, undulationes ætheris esse alias majores, sive crassiores aliis. Atque ita in cæteris. Hæc enim videtur esse summè necessaria lex & conditio hypothesium, in quibus naturalia corpora ponuntur constare ex quamplurimis corpusculis acervatim contextis, ut à diversis lucentium corpusculis, vel ejusdem corpusculi diversis partibus (prout motu, figurâ, mole, aut aliis qualitatibus differunt) inæquales pressiones, motiones aut mota corpuscula per æthera quaquaverfum trajiciantur, ex quibus confusè mistis lux constitui supponetur. Et nihil durius esse potest in istis hypothesibus quàm contraria suppositio.

Ex aperturâ sive dilatatione lucis in posteriori facie prismatis, quam R. P. dixit esse veluti foramen, sufficit, quod error non emerget sensibilis, si modo aliquis emerget. Quod si calculus juxta observationes præcisè ineatur, error erit nullus. Nam diametro foraminis à longitudine imaginis subductâ; restabit longitudo, quam imago haberet, si modo foramen ante prisma esset indivisibile, idque non obstante præfatâ lucis dilatatione in posteriori facie prismatis; ut facilè ostenditur. Deinde, ex datâ illâ longitudine imaginis, ac distantia à foramine indivisibili, ut & positione & formâ prismatis, & ad id inclinatione incidentium radiorum, ac angulo, quem refracti radii ad medium imaginis tendentes, cum à centro solis incidentibus constituunt, cætera omnia determinantur. Et quæ determinant refractiones & positiones radiorum, sufficiunt ad calculum istarum refractionum ritè ineundum. Sed res non tanti esse videtur, ut moram inferat.

Quod R. P. doctrinam nostram hypothesin vocaverit, non aliunde factum esse credo, quàm quod vocabulum usurpavit, quod primum occurrit; siquidem mos obtinuit, ut quicquid exponitur in Philosophiâ dicatur hypothesis. Et ego sane non alio consilio vocabulum istud reprehendi; quàm ut ne invalesceret appellatio, quæ rectè philosophantibus præjudicio esse posset. Reverend. verò Patris candor in omnibus conspicitur; indeque modus effrendi benevolentiam, qui mihi minimè convenit. Quod tamen nostra non displicent vehementer gaudeo Vale. Dabam Cantabrigiæ II. Junii 1672.

V.

To Mr. O L D E N B U R G .

S I R ,

July , 1672.

Of the true
method of
examining
this Theory.

IN the mean while give me leave, Sir, to insinuate, that I cannot think it effectual for determining truth, to examine the several ways by which phænomena may be explained, unless where there can be a perfect enumeration of all those ways. You know, the proper method for enquiring after the properties of things, is to deduce them from experiments. And I told you, that the theory which I propounded, was evinced to me, not by inferring, *it is thus, because it is not otherwise*; that is, not by deducing it only from a confutation of contrary suppositions, but by deriving it from experiments concluding positively and directly. The way therefore to examine it, is, by considering whether the experiments, which I propound, do prove those parts of the theory to which they are applied; or by prosecuting other experiments which the theory may suggest for its examination. And this I would have done in a due method; the laws of refraction being thoroughly enquired into and determined, before the nature of colours be taken into consideration. It may not be amiss to proceed according to the series of these Queries; which I could wish were determined by the event of proper experiments, declared by those that may have the curiosity to examine them.

1. Whether rays, that are alike incident on the same medium, have unequal refractions? And how great are the inequalities of their refractions at any incidence?

2. What is the law, according to which each ray is more or less refracted; whether it be, that the same ray is ever refracted according to the same ratio of the sines of incidence and refraction; and divers rays, according to divers ratios; or that the refraction of each ray is greater or less without any certain rule? That is, whether each ray have a certain degree of refrangibility, according to which its refraction is performed; or is refracted without that regularity?

3. Whether

3. Whether rays, which are endued with particular degrees of refrangibility, when they are by any means separated, have particular colours constantly belonging to them; *viz.* the least refrangible, Scarlet; the most refrangible, deep Violet; the middle, Sea-Green; and others, other colours? And on the contrary?

4. Whether the colour of any sort of rays apart may be changed by refraction?

5. Whether colours by coalescing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, indued with all variety of colours, produces light perfectly like that of the sun, and which hath all the same properties, and exhibits the same phænomena?

7. Whether the component colours of each mixture be really changed; or be only separated, when from that mixture various colours are produced again by refraction?

8. Whether there be any other colours produced by refraction, than such as ought to result from the colours belonging to the diversly refrangible rays, by their being separated or mixed by that refraction?

To determine by experiments these and such like queries, which involve the propounded theory, seems the most proper and direct way to a conclusion. And therefore I could wish all objections were suspended taken from hypotheses, or any other heads than these two: of shewing the insufficiency of experiments to determine these queries, or prove any other parts of my theory, by assigning the flaws and defects in my conclusions drawn from them; or of producing other experiments, which directly contradict me, if any such may seem to occur. For if the experiments, which I urge, be defective, it cannot be difficult to shew the defects; but if valid, then by proving the theory, they must render all objections invalid.

S I R ,

Cambridge,
July 11, 1672.

I HAVE already told you, that at the perusal of Mr. *Hook's* *Considerations* on my letter concerning Refractions and Colours, I found nothing that, as I conceived, might not without difficulty be answered. But, I must confess, at the first receipt of those considerations I was a little troubled to find a person so much concerned for an *hypothesis*, from whom in particular I most expected an unconcerned and indifferent examination of what I propounded. But yet I doubt not we have one common design; a sincere endeavour after knowledge, without valuing uncertain speculations for their subtleties, or despising certainties for their plainness. And on confidence of this it is, that I make this return to his Discourse.

Of the improvement of
Telescopes.

The first thing that offers itself is less agreeable to me, and I begin with it because it is so. Mr. *Hook* thinks himself concerned to reprehend me for laying aside the thoughts of improving optics by refractions. But he knows well it is not for one man to prescribe rules to the studies of another, especially not without understanding the grounds on which he proceeds. Had he obliged me by a private letter on this occasion, I would have acquainted him with my successes in the trials that I have made in that kind; which I shall now say have been less than I sometimes expected, and perhaps less than he at present hopes for. But since he is pleased to take it for granted, that I have let this subject pass without due examination, I must refer him to my former letter, by which that conjecture will appear to be ungrounded; for what I said there, was in respect of telescopes of the ordinary construction, signifying, that their improvement is not to be expected from the well figuring of glasses, as opticians have imagined. But I despaired not of their improvement by other constructions, which made me cautious to insert nothing that might intimate the contrary. For although successive refractions, which are all made the same way, do necessarily more and more augment the errors of the first refraction; yet it seemed

not impossible for contrary refractions so to correct each other's inequalities, as to make their difference regular; and if that could be conveniently effected, there would be no further difficulty. Now to this end I examined what may be done not only by glasses alone, but more especially by a complication of divers successive mediums; as by two or more glasses or crystals with water, or some other fluid between them; all which together may perform the office of one glass, especially of the object-glass on whose construction the perfection of the instrument chiefly depends. But what the results in theory, or by trials, have been, I may possibly find a more proper occasion to declare.

To the assertion, that rays are less true reflected to a point by a concave than refracted by a convex, I cannot assent; nor do I understand, that the focus of the latter is less a line than that of the former. The truth of the contrary you will rather perceive by this following Table, computed for such a reflecting concave and refracting convex; on supposition that they have equal apertures, and collect parallel rays at an equal distance from their vertex; which distance being divided into 15000 parts, the diameter of the concave sphere will be 60000 of those parts, and of the convex 10000; supposing the sines of incidence and refraction to be in round numbers, as 2 to 3. And this Table shews how much the exterior rays, at several apertures, fall short of their principal focus.

The diameter of the Aperture.	The parts of the Axis intercepted between the vertex and the rays		The error by	
	reflected.	refracted.	Reflection.	Refraction.
2000	14991 $\frac{1}{2}$	14865	8 $\frac{1}{2}$	135
4000	14966	14449	33	551
6000	14924	13699	76	1301
8000	14865	12475	135	2525
10000	14787	9472	213	5528

By this you may perceive, that the errors of the refracting convex are so far from being less, that they are more than fifteen times greater than the like errors of the reflecting concave, especially in great apertures, and that without respect to the heterogeneous constitution of light: so that however the contrary supposition might make Mr. *Hook* reject reflexions, as useless for the promoting of opticks, yet I must for this, as well as other

considerations, prefer them in the theory before refractions. Whether the parabola be more difficult to describe than the hyperbola or ellipsis, may be a query; but I see no absolute necessity of endeavouring after any of their descriptions. For if metals can be ground truly spherical, they will bear as great apertures as, I believe, men will be well able to communicate an exact polish to. And for dioptrick telescopes I told you, that the difficulty consisted not in the figure of the glass, but in the difformity of refractions; which, if it did not, I could tell you a better, and more easy remedy, than the use of the conic sections.

Reply to
Mr. Hook's
objections to
the Theory.

Thus much concerning the practice part of opticks. I shall now take a view of Mr. *Hook's* considerations on my theories. And those consist in ascribing an hypothesis to me, which is not mine; in asserting an hypothesis, which, as to the practical parts of it, is not against me; in granting the greatest part of my discourse, if explicated by that hypothesis; and in denying some things, the truth of which would have appeared by an experimental examination.

The hypothesis of the corporeity of Light no necessary foundation of the Newtonian Theory.

Of these particulars I shall discourse in order; and first of the hypothesis, which Mr. *Hook* hath assigned me in these words: *But grant his first supposition, that light is a body, and that as many colours or degrees thereof as there may be, so many bodies there may be, all which compounded together would make white, &c.* This, it seems, Mr. *Hook* takes for my hypothesis. It is true, that from my theory I argue the corporeity of light, but I do it without any absolute positiveness, as the word *perhaps* intimates, and make at most but a very plausible consequence of the doctrine, and not a fundamental supposition, nor so much as any part of it, which was wholly comprehended in the precedent propositions. And I wonder how Mr. *Hook* could imagine, that when I had asserted the theory with the greatest rigor, I should be so forgetful, as afterwards to assert the fundamental supposition itself with no more than a *perhaps*. Had I intended any such hypothesis, I should somewhere have explained it. But I knew that the properties, which I declared of light, were in some measure capable of being explicated not only by that, but by many other mechanical hypotheses; and therefore I chose to decline.

decline them all, and speak of light in general terms, considering it abstractedly as something or other propagated every way in streight lines from luminous bodies, without determining what that thing is; whether a confused mixture of difform qualities, or modes of bodies, or of bodies themselves; or of any virtues, powers or beings whatsoever. And for the same reason I chose to speak of colours according to the information of our senses, as if they were qualities of light without us. Whereas by that hypothesis, I must have considered them rather as modes of sensation, excited in the mind by various motions, figures, or sizes of the corpuscles of the light, making various mechanical impressions on the organs of sense, as I expressed it in that place where I spake of the corporeity of light.

But supposing I had propounded this hypothesis, I understand not why Mr. *Hook* should so much endeavour to oppose it. For certainly it hath a much greater affinity with his own hypothesis, than he seems to be aware of; the vibrations of æther being as useful and necessary in this, as in his own. For assuming the rays of light to be small bodies, emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the æther, as stones do in water, when thrown into it. And supposing these vibrations to be of several depths or thickneses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities; of what use they will be for explicating the manner of reflexion and refraction, the production of heat by the sun's beams, the emission of light from burning, putrifying, or other substances, whose parts are vehemently agitated, the phænomena of thin transparent plates and bubbles, and of all natural bodies, the manner of vision, and the difference of colours, as also their harmony and discord; I shall leave to their consideration, who may think it worth their endeavour to apply this hypothesis to the solution of phænomena.

In the second place I told you, that Mr. *Hook's* hypothesis, as to the fundamental part of it, is not against me. The fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the æther, which are propagated every way from those bodies in streight lines, and cause a sensation

Its near affinity with Hook's hypothesis.

Of Hook's hypothesis. Conformity of that and of every mechanical hypothesis with the Newtonian doctrine.

tion of light, by beating and dashing against the bottom of the eye; something after the manner that vibrations in the air cause a sensation of sound, by beating against the organs of hearing. Now the most free and natural application of this hypothesis to the solution of phænomena I take to be this: That the agitated parts of bodies, according to their several sizes, figures, and motions, excite vibrations in the æther of various depths or bignesses; which, being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour: but if by any means those of unequal bignesses be separated from one another, the largest, a sensation of a Red colour; the least, or shortest, of a deep Violet; and the intermediate ones, of intermediate colours. Much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which, according to those bignesses, make several tones in sound. That the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction: whence the vibrations of several bignesses, that is, the rays of several colours, which are blended together in light, must be parted from one another by refraction; and so cause the phænomena of prisms, and other refracting substances. And that it depends on the thickness of a thin transparent plate or bubble, whether a vibration shall be reflected at its further superficies, or transmitted; so that according to the number of vibrations interceding the two superficies, they may be reflected or transmitted for many successive thicknesses. And since the vibrations, which make Blue and Violet, are supposed shorter than those that make Red and Yellow; they must be reflected at a less thickness of the plate, which is sufficient to explicate all the ordinary phænomena of those plates or bubbles; and also of all natural bodies, whose parts are like so many fragments of such plates. These seem to be the most plain, genuine, and necessary conditions of this hypothesis; and they agree so justly with my theories, that, if Mr. *Hook* think fit to apply them, he need not on that account fear a divorce from it. But yet how he will defend it from other difficulties, I know not; for to me the fundamental supposition it-

self

self seems impossible; namely, that the waves or vibrations of any fluid can, like the rays of light, be propagated in straight lines, without a continual and very extravagant spreading and bending every way into the quiescent medium, where they are terminated by it. I am mistaken if there be not both experiment and demonstration to the contrary: and as to the other two or three hypotheses which he mentions, I had rather believe them subject to the like difficulties, than suspect that Mr. *Hook* should select the worst for his own.

What I have said of this, may be easily applied to all other mechanical hypotheses, in which light is supposed to be caused by any pressure or motion whatsoever, excited in the æther by the agitated parts of luminous bodies. For it seems impossible, that any of those motions or pressures can be propagated in straight lines, without the like spreading every way into the shadowed medium, on which they border. But yet, if any man can think it possible, he must at least allow, that those motions, or endeavours to motion, caused in the æther by the several parts of any lucid bodies, which differ in size, figure, and agitation, must necessarily be unequal; which is enough to denominate light an aggregate of difform rays, according to any of those hypotheses. And if those original inequalities may suffice to difference the rays in colour and refrangibility, I see no reason why they, that adhere to any of those hypotheses, should seek for other causes of these effects, unless (to use Mr. *Hook's* argument) they will multiply entities without necessity.

The third thing to be considered, is, the condition of Mr. *Hook's* concessions, which is, that I would explicate my theories by his hypothesis: and if I could but comply with him in that point, there would be little or no difference between us: for he grants, that without any respect to a different incidence of rays, there are different refractions; but he would have it explicated, not by the different refrangibility of several rays, but by the splitting and rarifying of æthereal pulses. He grants my third, fourth, and sixth Propositions; the sense of which is, that uncompounded colours are unchangeable, and that compounded colours are changeable, only by resolving them into the colours of which they are compounded; and that all the changes,

Of *Hook's* concessions, and the limitations of them to his hypothesis.

changes, which can be wrought in colours, are effected only by variously mixing or parting them: but he grants them, on condition that I will explicate colours by the two sides of a split pulse, and so make but two species of them, accounting all other colours in the world to be but various degrees and dilutions of those two. And he further grants, that whiteness is produced by the convention of all colours; but then I must allow it to be not only by mixture of those colours, but by a further uniting of the parts of the ray supposed to be formerly split. If I would proceed to examine these his explications, I think it would be no difficult matter to shew, that they are not only insufficient, but in some respects unintelligible. For though it be easy to conceive how motion may be dilated and spread, or how parallel motions may become diverging; yet I understand not by what artifice any linear motion can, by a refracting superficies, be infinitely dilated and rarified so as to become superficial: or if that be supposed, yet I understand as little why it should be split at so small an angle only, and not rather spread, and dispersed through the whole angle of refraction. And, further, though I can easily imagine how unlike motions may cross one another, yet I cannot well conceive how they should coalesce into one uniform motion, and then part again, and recover their former unlikeness; notwithstanding that I conjecture the ways by which Mr. *Hook* may endeavour to explain it. So that the direct, uniform, and undisturbed pulses, should be split and disturbed by refraction; and yet the oblique and disturbed pulses persist without splitting, or further disturbance, by following refractions, is as unintelligible. And there is as great difficulty in the number of colours, as you will see hereafter.

The Newtonian doctrine independent of every hypothesis.

But whatever be the advantages or disadvantages of this hypothesis, I hope Mr. *Hook* will excuse me from taking it up, since I do not think it needful to explicate my doctrine by any hypothesis at all; for if light be considered abstractedly without respect to any hypothesis, I can as easily conceive, that the several parts of a shining body may emit rays of different colours, and other qualities, of all which light is constituted, as that the several parts of a false or uneven string, or of agitated water in a
brook

brook or a cataract, or the several pipes of an organ inspired all at once, or all the variety of sounding bodies in the world together, should produce sounds of several tones, and propagate them through the air confusedly intermixed. And if there were any natural bodies, which could reflect sounds of one tone, and stifle or transmit those of another; then, as the echo of a confused aggregate of all tones would be that particular tone, which the echoing body is disposed to reflect: so since even by Mr. *Hook's* concessions there are bodies apt to reflect rays of one colour, and stifle or transmit those of another; I can as easily conceive, that those bodies, when illuminated by mixture of all colours, must appear of that colour only which they reflect.

But when Mr. *Hook* would insinuate a difficulty in these things, by alluding to sounds in the string of a musical instrument before percussion, or in the air of an organ-bellows before its arrival to the pipes: I must confess I understand it as little, as if he had spoke of light in a piece of a wood, before it be set on fire; or in the oil of a lamp, before it ascend up the match to feed the flame.

You see therefore how much it is besides the business in hand, to dispute about hypothesis. For which reason I shall now, in the last place, proceed to abstract the difficulties involved in Mr. *Hook's* discourse, and, without having regard to any hypothesis, consider them in general terms. And they may be reduced to these queries: Whether the unequal refractions, made without respect to any inequality of incidence, be caused by the different refrangibility of several rays, or by the splitting, breaking, or dissipating the same ray into diverging parts? Whether there be more than two sorts of colours; and whether whiteness be a mixture of all colours?

The first of these queries you may find already determined by an experiment in my former letter: the design of which was to shew, that the length of the coloured image proceeded not from any unevenness in the glass, or any other contingent irregularity in the refractions. Amongst other irregularities, I know not what is more obvious to suspect, than a fortuitous dilating and spreading of light, after some such manner as *Des Cartes* hath described

described in his æthereal refractions for explicating the tail of a comet; or as Mr. *Hook* now supposeth to be effected by the splitting and rarifying of his æthereal pulses. And to prevent the suspicion of any such irregularities, I told you, that I refracted the light contrary ways with two prisms successively; to destroy thereby the regular effects of the first prism by the second; and to discover the irregular effects, by augmenting them with iterated refractions. Now, amongst other irregularities, if the first prism had spread and dissipated every ray into an indefinite number of diverging parts; the second should, in like manner, have spread and dissipated every one of those parts into a further indefinite number: whereby the image would still be more dilated, contrary to the event. And this ought to have happened, because those linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly and compleatly rays, as the whole was before its incidence, as may appear by intercepting them severally.

The reasonableness of this proceeding will, perhaps, better appear, by acquainting you with this further circumstance. I sometimes placed the second prism in a position transverse to the first, on design to try if it would make the long image become four-square, by refractions crossing those which had drawn the round image into a long one. For if amongst other irregularities the refraction of the first prism did, by splitting, dilate a linear ray into a superficial, the cross refractions of that second prism ought, by further splitting, to dilate, and draw that superficial ray into a pyramidal solid. But upon trial I found it otherwise; the image being as regularly oblong as before, and inclined to both the prisms at an angle of 45 degrees.

I tried also all other positions of the second prism, by turning the ends about its middle part, and in no case could observe any such irregularity. The image was ever alike inclined to both prisms, its breadth answering to the sun's diameter, and its length being greater or less, accordingly as the refractions more or less agreed or contradicted one another. And by these observations, since the breadth of the image was not augmented by the cross refraction of the second prism, that refraction must have been performed

performed without any splitting and dilating of the ray: and therefore at least the light incident on that prism must be granted an aggregate of rays unequally refrangible in my sense. And since the image was equally inclined to both prisms, and consequently the refractions alike in both, it argues, that they were performed according to some constant law without any irregularity.

To determine the second query, Mr. *Hook* refers to an experiment made with two wedge-like boxes; the design of which was to produce all colours out of a mixture of two. But there is a double defect in this instance; for it appears not that by this experiment all colours can be produced out of two; and if they could, yet the inference would not follow. That all colours cannot, by the experiment, be produced out of two, will appear by considering, that the tincture of *aloes*, which afforded one of those colours, was not all over of one uniform colour, but appeared Yellow near the edge of the box, and Red at other places where it was thicker, affording all variety of colours from a pale Yellow to a deep Red or Scarlet, according to the various thickness of the liquor. And so the solution of copper, which afforded the other colour, was of various Blues and Indicos: so that instead of two colours, here is a great variety made use of, for the production of all colours. Thus, for instance, to produce all sorts of Greens, the several degrees of Yellow and pale Blue must be mixed; but to compound Purples, the Scarlet and deep Blue or Indico are to be the ingredients.

Now if Mr. *Hook* contend, that all the Reds and Yellows of the one liquor, or Blues and Indicos of the other, are only various degrees and dilutings of the same colour, and not divers colours; that is a begging of the question: and I should as soon grant, that the two-thirds or sixths in music are but several degrees of the same sound, and not divers sounds. Certainly it is much better to believe our senses informing us, that Red and Yellow are divers colours; and to make it a philosophical query, why the same liquor doth, according to its various thickness, appear of those divers colours; than to suppose them to be the same colour, because exhibited by the same liquor. For if that were

the sufficient reason, then Blue and Yellow must also be the same colour, since they are both exhibited by the same tincture of *ne-
britick* wood.

But that they are divers colours, you will more fully understand by the reason of them, which is this: The tincture of aloes is qualified to transmit most easily the rays indued with Red; most difficulty, the rays indued with Violet; and with intermediate degrees of facility, the rays indued with intermediate colours. So that where the liquor is very thin, it may suffice to intercept most of the Violet, and yet transmit most of the other colours; all which together must compound a middle colour, that is, a faint Yellow. And where it is so much thicker, as also to intercept most of the Blue and Green; the remaining Green, Yellow, and Red, must compound an Orange. And where the thickness is so great, that scarce any rays can pass through it, besides those indued with Red; it must appear of that colour, and that so much the deeper and obscurer, by how much the liquor is thicker. And the same may be understood of the various degrees of Blue exhibited by the solution of copper, by reason of its disposition to intercept Red most easily, and transmit a deep Blue or Indico colour most freely.

But supposing that all colours might, according to this experiment, be produced out of two by mixture; yet it follows not, that those two are the only original colours, and that for a double reason. First, because those two are not themselves original colours, but compounded of others; there being no liquor, nor any other body in nature, whose colour in day-light is wholly uncompounded. And then, because though those two were original, and all others might be compounded of them; yet it follows not that they cannot be otherwise produced. For I said, that they had a double origin, the same colours to sense being in some case compounded, and in others uncompounded; and sufficiently declared in my third and fourth Propositions, and in the conclusion, by what properties the one might be known and distinguished from the other. But because I suspect, by some circumstances, that the distinction might not be rightly apprehended; I shall once more declare it, and further explain it by examples.

That

That colour is *primary, or original, which cannot by any art* Rule to distinguish compounded and uncompounded colours.
be changed, and whose rays are all alike refrangible; and that compounded, which is changeable into other colours, and whose rays are not alike refrangible. For instance, to know whether the

colour of any Green object be compounded or not, view it through a prism; and if it appear confused, and the edges tinged with Blue, Yellow, or any variety of other colours, then is that Green compounded of such colours, as at its edges emerge out of it. But if it appear distinct and well defined, and intirely Green to the very edges, without any other colours emerging; it is of an original and uncompounded Green. In like manner, if a refracted beam of light, being cast on a white wall, exhibit a Green colour; to know whether that be compounded, refract the beam with an interposed prism: and if you find any difformity in the refractions, and the Green be transformed into Blue, Yellow, or any variety of other colours; you may conclude, that it was compounded of those colours which emerge. But if the refraction be uniform, and the Green persist without any change of colour, then it is original and uncompounded. And the reason why I call it so, is, because a Green, indued with such properties, cannot be produced by any mixing of other colours.

Now if two green objects may, to the naked eye, appear of the same colour; and yet one of them, through a prism, seem confused, and variegated with other colours at the edges; and the other distinct and intirely Green: or, if there may be two beams of light which, falling on a white wall, do, to the naked eye, exhibit the same Green colour; and yet one of them, when transmitted through a prism, be uniformly and regularly refracted, and retain its colour unchanged; and the other be irregularly refracted, and made to divaricate into a multitude of other colours: I suppose these two Greens will, in both cases, be granted of a different origin and constitution. And if by mixing colours a Green cannot be compounded with the properties of the unchangeable Green, I think I may call that an uncompounded colour; especially since its rays are all alike refrangible, and uniform in all respects.

The

The same rule is to be observed in examining whether Red, Orange, Yellow, Blue, or any other colour, be compounded or not. And, by the way, since all White objects through the prism appeared confused, and terminated with colours; Whiteness must, according to this distinction, be ever compounded; and that the most of all colours, because it is the most confused and changed by refractions.

A way of
improving
Microscopes.

From hence I may take occasion to communicate a way for the improvement of microscopes by refraction; which I do the more willingly, because Mr. *Hook* hath made such excellent use of that instrument; and I shall be glad if it will contribute any thing to your promotion of those his ingenious endeavours, or add to his inventions of that kind. The way is, by illuminating the object in a darkened room with light of any convenient colour, not too much compounded. For by that means the microscope will, with distinctness, bear a deeper charge and larger aperture; especially if its construction be such as I may hereafter describe: for the advantage in ordinary microscopes will not be so sensible.

Whiteness a
mixture of all
colours.

There remains now the third query to be considered; and that is, whether Whiteness be an uniform colour, or a dissimilar mixture of all colours. The experiment which I brought to decide it, Mr. *Hook* thinks may be otherwise explained, and so concludes nothing. But he might easily have satisfied himself, by trying what would be the result of a mixture of all colours. And that very experiment might have satisfied him, if he had examined it by the various circumstances. One circumstance I there declared, of which I see no notice taken; and it is, that if any colour at the lens be intercepted, the whiteness will be changed into the other colours. If all the colours but Red be intercepted, the Red alone, in the concurrence or crossing of the rays, will not constitute whiteness, but continues as much Red as before; and so of the other colours. So that the business is not only to shew how rays, which before the concurrence exhibit colours, do in the concurrence exhibit White: but to shew how, in the same place where the several sorts of rays apart exhibit several colours, a confusion of all together make White. For instance, if Red alone be first transmitted to the paper at the place
of

of concurrence, and then the other colours be let fall on that Red: the question will be, whether they convert it into White, by mixing with it only; as Blue, falling upon Yellow light, is supposed to compound Green: or whether there be some further change wrought in the colours by their mutual acting on one another, until, like contrary peripatetic qualities, they become assimilated. And he that shall explicate this case mechanically, must conquer a double impossibility. He must first shew, that many unlike motions in a fluid can, by clashing, so act on one another, and change each other, as to become one uniform motion; and then, that an uniform motion can of itself, without any new unequal impressions, depart into a great variety of motions regularly unequal. And after this he must further tell me, why all objects appear not of the same colour; that is, why their colours in the air, where the rays that convey them every way are confusedly mixed, do not assimilate one another, and become uniform, before they arrive at the spectator's eye.

But if there be yet any doubting, it is better to put the event on further circumstances of the experiment, than to acquiesce in the possibility of any hypothetical explication. As, for instance, by trying what will be the apparition of these colours in a very quick consecution of one another. And this may be easily performed by the rapid gyration of a wheel with many spokes, or cogs in its perimeter, whose interstices and thickneses may be equal; and of such a largeness, that if the wheel be interposed between the prism and the White concurrence of the colours, one half of the colours may be intercepted by a spoke or cog, and the other half pass through an interstice. The wheel being in this posture, you may first turn it slowly about, to see all the colours fall successively on the same place of the paper, held at the aforesaid concurrence: and if you then accelerate its gyration, until the consecution of those colours be so quick, that you cannot distinguish them severally; the resulting colour will be a whiteness perfectly like that which an unrefracted beam of light exhibiteth, when in like manner successively interrupted by the spokes or cogs of that circulating wheel. And that this whiteness is produced only by a successive intermixture of the colours,
without

without their being assimilated or reduced to any uniformity, is certainly beyond all possibility of doubting; unless things that exist not at the same time, may notwithstanding act on one another.

There are yet other circumstances, by which the truth might have been decided; as by viewing the White concourse of the colours through another prism, placed close to the eye, by whose refraction that whiteness may appear again transformed into colours. And then, to examine their origin, if an assistant intercept any of the colours at the lens before their arrival at the whiteness, the same colours will vanish from amongst those, into which that whiteness is converted by the second prism. Now if the rays which disappear be the same with those that are intercepted; then it must be acknowledged, that the second prism makes no new colour in any rays which were not in them before their concourse at the paper: which is a plain indication, that the rays of several colours remain distinct from one another in the whiteness, and that from their previous dispositions are derived the colours of the second prism. And, by the way, what is said of their colours, may be applied to their refrangibility. The aforesaid wheel may be also here made use of. And if its gyration be neither too quick nor too slow, the succession of the colours may be discerned through the prism, whilst, to the naked eye of a by-stander, they exhibit whiteness.

There is something still remaining to be said of this experiment. But this I conceive is enough to enforce it, and so to decide the controversy. However, I shall now proceed to shew some other ways of producing whiteness by mixtures; since I persuade myself, that this assertion above the rest appears paradoxical, and is with most difficulty admitted. And because Mr. *Hook* desires an instance of it in bodies of divers colours, I shall begin with that. But in order thereto, it must be considered, that such coloured bodies reflect but some part of the light incident on them, as is evident by the 13th Proposition; and therefore the light, reflected from an aggregate of them, will be much weakened by the loss of many rays. Whence a perfect and intense whiteness is not to be expected, but rather a colour
6 between

between those of light and shadow, or such a Grey or dirty colour as may be made by mixing White and Black together. And yet such a colour will result, may be collected from the colour of dust found in every corner of a house, which hath been observed to consist of many coloured particles. There may be also produced the like dirty colour, by mixing several painter's colours together. And the same may be effected by painting a top (such as boys play with) of divers colours; for when it is made to circulate by whipping, it will appear of such a dirty colour.

Now the compounding of these colours is proper to my purpose, because they differ not from whiteness in the species of colours, but only in degrees of luminousness, which, did not Mr. *Hook* concede it, I might thus evince. A beam of the sun's light being transmitted into a darkened room, if you illuminate a sheet of white paper by that light reflected from a body of any colour; the paper will always appear of the colour of that body, by whose reflected light it is illuminated. If it be a Red body, the paper will be Red; if a Green, it will be Green; and so of the other colours. And the reason is, that the fibres or threads, of which the paper consists, are all transparent and specular; and such substances are known to reflect colours without changing them. To know, therefore, to what series of colours a Grey belongeth, place any Grey body, suppose a mixture of painted colours, in the said light; and the paper, being illuminated by its reflection, shall appear White; and the same thing will happen, if it be illuminated by reflection from a Black substance.

These, therefore, are all of one species; but yet they seem to be distinguished not only by degrees of luminousness, but also by some other inequalities, whereby they become more harsh or pleasant: and the distinction seems to be, that Grays, and perhaps Black, are made by an uneven defect of light, consisting as it were of many little veins or streams, which differ either in luminousness, or in the unequal distribution of diversly coloured rays, such as ought to be caused by reflection from a mixture of White and Black, or of diversly coloured corpuscles. But when such imperfectly mixed light is, by a second reflection from the paper, more evenly and uniformly blended, it becomes more
VOL. IV. X x pleasant,

Greys not specifically different from White.

Greys and Black by what distinguished from White.

pleasant, and exhibits a faint or shadowed whiteness. And that such little irregularities as these may cause these differences is not improbable, if we consider how much variety may be caused in sounds of the same tone by irregular and uneven jarrings. And, besides, these differences are so little, that I have sometimes doubted whether they be any at all; when I have considered, that a Black and a White body being placed together, the one in a strong light, and the other in a very faint light so proportioned, that they might appear equally luminous; it hath been difficult to distinguish them, when viewed at a distance, unless when the Black seemed more Blueish, and the White body in a light still fainter, hath, in comparison of the Black body itself, appeared Black.

Another way
of compound-
ing White-
ness.

This leads me to another way of compounding Whiteness, which is this: that if four or five bodies of the more eminent colours, or a paper painted all over in several parts of it with those several colours in a due proportion, be placed in the said beam of light; the light reflected from those colours to another white paper, held at a convenient distance, shall make that paper appear White. If it be held too near the colours, its parts will seem of those colours which are nearest them; but by removing it further, that all its parts may be equally illuminated by all the colours, they will be more and more diluted, until they become perfectly White. And you may further observe, that if any of the colours be intercepted, the paper will no longer appear White, but of the other colours which are not intercepted. Now that this Whiteness is a mixture of the severally coloured rays, falling confusedly on the paper, I see no reason to doubt of; because if the light became uniform and similar before it fell on the paper, it must much more be uniform, when at a greater distance it falls on the spectator's eye; and so the rays which come from several colours would, in no qualities, differ from one another, but all of them exhibit the same colour to the spectator, contrary to what he sees. Not much unlike this instance it is, that if a polished piece of metal be so placed, that the colours appear in it as a looking-glass, and then the metal be made rough, that by a confused reflection those apparent colours may be blended together,

gether, they shall disappear, and by their mixture cause the metal to look White.

But further to enforce this experiment, if instead of the paper any white froth, consisting of small bubbles, be illuminated by reflection from the aforesaid colours, it shall to the naked eye seem White; and yet through a good microscope the several colours will appear distinct on the bubbles, as if seen by reflection from so many spherical surfaces; with my naked eye being very near, I have also discerned the several colours on each bubble; and yet at a greater distance, where I could not distinguish them apart, the froth hath appeared entirely White. And at the same distance, when I looked intently, I have seen the colours distinctly on each bubble; and yet by straining my eyes, as if I would look at something far off beyond them, thereby to render the vision confused, the froth hath appeared without any other colour than Whiteness. And what is here said of froth, may be easily understood of the paper or metal in the foregoing experiments; for their parts are specular bodies, like those bubbles, and perhaps with an excellent microscope the colours may be also seen intermixedly reflected from them.

In proportioning the severally coloured bodies to produce these effects, there may be some niceness; and it will be more convenient to make use of the colours of the prism cast on a wall, by whose reflection the paper, metal, froth, and other white substances may be illuminated: and I usually made my trials this way, because I could better exclude any scattering light from mixing with the colours to dilute them.

To this way of compounding Whiteness may be referred that other, by mixing light after it hath been trajected through transparently coloured substances. For instance, if no light be admitted into a room but only through coloured glass, whose several parts are of several colours in a pretty equal proportion, all white things in the room shall appear White, if they be not held too near the glass: and yet this light with which they are illuminated cannot possibly be uniform, because if the rays, which at their entrance are of divers colours, do, in their progress through the room, suffer any alteration to be reduced to an

uniformity, the glass would not, in the remotest parts of the room, appear of the very same colour which it doth when the spectator's eye is very near it. Nor would the rays, when transmitted into another dark room through a little hole in an opposite door or partition-wall, project on a paper the species or representation of the glass in its proper colours.

And, by the way, this seems a very fit and cogent instance of some other parts of my theory, and particularly of the 13th Proposition. For in this room all natural bodies whatever appear of their proper colours; and all the phænomena of colours in nature, made either by refraction or without it, are here the same as in the open air. Now the light in this room being such a dissimilar mixture as I have described in my Theory, the causes of all these phænomena must be the same that I have there assigned. And I can see no reason to suspect, that the same phænomena should have other causes in the open air. The success of this experiment may be easily conjectured by the appearance of things in a church or chapel, whose windows are of coloured glass; or in the open air, when it is illustrated with clouds of various colours.

There are yet other ways by which I have produced Whiteness: as by casting several colours from two or more prisms upon the same place, by refracting a beam of light with two or three prisms successively, to make the diverging colours converge again; by reflecting one colour upon another; and by looking through a prism on an object of many colours; and, which is equivalent to Mr. Hook's way of mixing colours, by concave-wedges filled with coloured liquors, I have observed the shadows of a painted glass-window to become white, where those of many colours have at a great distance interfered. But yet, for further satisfaction, he may try, if he please, the effects of four or five such wedges filled with liquors of as many colours.

Besides all these, the colours of water-bubbles, and other thin pellucid substances, afford several instances of Whiteness, produced by their mixture; with one of which I shall conclude this particular. Let some water, in which a convenient quantity of soap or wash-bal is dissolved, be agitated into a froth; and after that froth

hath

hath stood a while without further agitation, till you see the bubbles of which it consists begin to break, there will appear a great variety of colours all over the top of every bubble, if you view them near at hand; but if you view them at so great a distance, that you cannot distinguish the colours one from another, the froth will appear perfectly White.

Thus much concerning the design and substance of Mr. Hook's *Experimentum Crucis* Experimentum Crucis assumed. considerations. There are yet some particulars to be taken notice of; as the denial of the *Experimentum Crucis*. On this I chose to lay the whole stress of my discourse, which therefore was the principal thing to have been objected against. But I cannot be convinced of its insufficiency by a short bare denial, without assigning a reason for it. I am apt to believe it hath been misunderstood. For otherwise it would have prevented the discourses about rarefying and splitting of rays; because the design of it is to shew, that rays of divers colours, considered apart, do at equal incidences suffer unequal refractions, without being split, rarefied, or any way dilated.

In the considerations on my first and second Propositions, Mr. Hook's representation of the Newtonian doctrine very imperfect. Hook hath rendered my doctrine of unequal refrangibility very imperfect and maimed, by explicating it wholly by the splitting of rays; whereas I chiefly intended it in those refractions, which are performed without that supposed irregularity, such as the *Experimentum Crucis* might have informed him of. And in general I find, that whilst he hath endeavoured to explicate my Propositions hypothetically, the more material suggestions, by which I designed to recommend them, have escaped his consideration; such as are the unchangeableness of the degree of refrangibility peculiar to any sort of rays; the strict analogy between the degrees of refrangibility and colours; the distinction between compounded and uncompounded colours; the unchangeableness of uncompounded colours; and the assertion, that if any one of the prismatic colours be wholly intercepted, that colour cannot be new produced out of the remaining light by any further refraction or reflection whatsoever; and of what length and efficacy these particulars are for enforcing the theory, I desire, therefore, may be now considered.

In

T H E O R Y O F

In the last place, I should take notice of a casual expression, which intimates a greater certainty in these things, than I ever promised, viz. *the certainty of Mathematical Demonstrations*. I said, indeed, that the science of colours was mathematical, and as certain as any other part of Optics; but who knows not that Optics, and many other mathematical sciences, depend as well on physical sciences, as on mathematical demonstrations? And the absolute certainty of a science cannot exceed the certainty of its principles. Now the evidence, by which I asserted the propositions of colours, is in the next words expressed to be from experiments, and so but physical: whence the Propositions themselves can be esteemed no more than physical principles of a science. And if those principles be such, that on them a mathematician may determine all the phenomena of colours, that can be caused by refractions, and that by disputing or demonstrating after what manner, and how much, those refractions do separate or mingle the rays, in which several colours are originally inherent; I suppose the science of colours will be granted mathematical, and as certain as any part of Optics. And that this may be done, I have good reason to believe, because ever since I became first acquainted with these principles, I have, with constant success in the events, made use of them for this purpose.

Thus much I have thought fit to return to Mr. *Hook's* considerations, which, that it may bring satisfaction in this part of Optics to the honourable members of the Royal Society, hath been the rule of my intentions. Yours, &c.

V I I.

To Mr. O L D E N B U R G.

S I R,

Cambridge.

I Received your letter, with Mr. *Hugen's* kind present, which I have viewed with great satisfaction, finding it full of very subtle and useful speculation very worthy of the author. I am glad that we are to expect another discourse of the *vis centrifuga*, which speculation may prove of good use in natural philosophy and astronomy, as well as mechanics.

I

Thus,

L I G H T A N D C O L O U R S.

Thus, for instance, if the reason why the same side of the moon is ever towards the earth, be the greater *conatus* of the other side to recede from it; it will follow, upon supposition of the earth's motion about the sun, that the greatest distance of the sun from the earth is to the greatest distance of the moon from the earth not greater than 10000 to 56: and therefore the parallax of the sun will not be less than $\frac{56}{10000}$ of the parallax of the moon. Because were the sun's distance less in proportion to that of the moon, she would have a greater *conatus* from the sun than from the earth. I thought also sometimes, that the moon's libration might depend upon her *conatus* from the sun and earth compared together, till I apprehended a better cause.

In the demonstration of the 18th Proposition, *De descensu gravium*, there seems to be an illegitimate supposition; namely, that the flexures at B and C do not hinder the motion of the descending body. For in reality they will hinder it; so that the body, which descends from A, shall not acquire so great velocity when arrived at D, as one which descends from E. If this supposition be made, because a body descending by a curve line meets with no such opposition, and this proposition is laid down in order to the contemplation of motion in curve lines; then it should have been shewn, that though rectilinear flexures do hinder, yet the infinitely little flexures, which are in curves, though infinite in number, do not at all hinder the motion.

The rectifying curve lines by that way, which Mr. *Hugens* calls evolution, I have been some time considering also; and have met with a way of resolving it, which seems more ready and free from the trouble of calculation, than that of Mr. *Hugens*. If he please, I will send it him. The Problem also is capable of being improved, by being propounded thus more generally.

Curvas invenire qualescunque, quarum longitudines cum proposita alicujus Curvæ longitudine, vel cum arcu ejus, ad datam lineam applicatâ, comparari possunt.

Concerning the business of colours, when Mr. *Hugens* hath shewn how White may be produced out of two uncompound colours, I will tell him, why he can conclude nothing from that; my meaning

Elimination of
the sun's pa-
rallax by the
moon's.

An illegiti-
mate suppo-
sition in Mr.
Hugen's
demonstration
of his 18th
Prop. de
descensu
gravium.

Evolution.

White not to
be produced
out of two
uncompound-
ed Colours.

meaning was, that such a White, were there any such, would have different properties from the White which I had respect to, when I described my Theory; that is, from the White of the sun's immediate light, of the ordinary objects of our senses, and of all white phenomena that have hitherto fallen under my observation. And those different properties would evince it to be of a different constitution: inasmuch, that such a production of White would be so far from contradicting, that it would rather illustrate and confirm my Theory; because by the difference of that from other Whites, it would appear, that other Whites are not compounded of only two colours like that. And therefore if Monf. H. would prove any thing, it is requisite that he do not only produce out of two primitive colours a White, which to the naked eye shall appear like other Whites, but also shall agree with them in all other properties.

But to let you understand wherein such a White would differ from other Whites, and why from thence it would follow, that other Whites are otherwise compounded; I shall lay down this position.

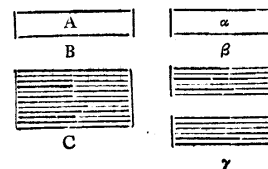
That a compounded colour can be resolved into no more simple colours, than those of which it is compounded.

This seems to be self-evident; and I have also tried it several ways, and particularly by this which follows. Let α represent an oblong piece of White paper, about $\frac{1}{3}$ or $\frac{1}{4}$ of an inch broad, and illuminated in a dark room, with a mixture of two colours cast upon it from two prisms; suppose a deep Blue and Scarlet, which must severally be as uncompounded as they can conveniently be made. Then, at a convenient distance, suppose of six or eight yards, view it through a clear triangular glass or crystal prism, held parallel to the paper; and you shall see the two colours parted from one another in the fashion of two images of the paper, as they are represented at β and γ ; where suppose β the Scarlet, and γ the Blue, without Green, or any other colour between them.

Now from the aforesaid position I deduce these two conclusions.
1. That if there were found out a way to compound White of two simple colours, that White would be again resolvable into no more

more than two. 2. That if other Whites, as that of the sun's light, &c. be resolvable into more than two simple colours, as I find by experiment that they are, then they must be compounded of more than two.

To make this plainer, suppose that A represents a White body, illuminated by a direct beam of the sun, transmitted through a small hole into a dark room; and α , such another body, illuminated by a mixture of two simple colours; which, if possible, may make it also appear of a White colour exactly like A. Then, at a convenient distance, view these two Whites



through a prism; and A will be changed into a series of all colours, Red, Yellow, Green, Blue, Purple, with their intermediate degrees succeeding in order from B to C. But α , according to the aforesaid experiment, will only yield those two colours of which it was compounded; and those not conterminate like the colours at BC, but separate from one another, as at β and γ , by means of the different refrangibility of the rays, to which they belong. And thus by comparing these two Whites, they would appear to be of a different constitution, and A to consist of more colours than α . So that what Monf. H. contends for, would rather advance my Theory by the access of a new kind of White, than conclude against it. But I see no hopes of compounding such a White.

As for Monf. H. his expression, *That I maintain my doctrine with some concern*; I confess it was a little ungrateful to me, to meet with objections which had been answered before, without having the least reason given me, why those answers were insufficient. Those answers were to shew, that there are other simple colours besides Blue and Yellow; I instanced in a simple or homogeneous Green, such as cannot be made by mixing Blue and Yellow, or any other colours. And I also shewed why, supposing that all colours might be produced out of two, yet it would not follow that those two are the only original colours. The reasons I desire you would compare with what hath now been said of White. And so the necessity of all colours to produce White, might

might have appeared by that experiment, where I say, That if any colour at the lens be intercepted, the Whiteness (which is compounded of them all) will be changed into (the result of) the other colours.

However, since there seems to have happened some misunderstanding between us, I shall endeavour to explain myself a little further in these things, according to the following method.

DEFINITIONS.] I. I call that Light homogeneal, similar or uniform, whose rays are equally refrangible.

II. And that heterogeneal, whose rays are unequally refrangible.

Note. There are but three affections of light, in which I have observed its rays to differ, *viz. refrangibility, reflexibility and colour*; and those rays which agree in refrangibility, agree also in the other two, and therefore may well be defined homogeneal; especially since men usually call those things homogeneal, which are so in all qualities that come under their knowledge, though in other qualities, that their knowledge extends not to, there may possibly be some heterogeneity.

III. Those colours I call simple, or homogeneal, which are exhibited by homogeneal light.

IV. And those compounded or heterogeneal, which are exhibited by heterogeneal light.

V. Different colours, I call not only the more eminent species, Red, Yellow, Green, Blue, Purple, but all other the minutest gradations; much after the same manner that not only the more eminent degrees of musick, but all the least gradations are esteemed different sounds.

PROPOSITIONS.] I. The sun's light consists of rays differing by indefinite degrees of refrangibility.

II. Rays which differ in refrangibility when parted from one another, do proportionally differ in the colours which they exhibit. These two Propositions are matter of fact.

III. There are as many simple or homogeneal colours, as degrees of refrangibility: for to every degree of refrangibility belongs

longs a different colour, by Prop. II. and that colour is simple, by Def. I. and III.

IV. Whiteness, in all respects like that of the sun's immediate light, and of all the usual objects of our senses, cannot be compounded of two simple colours alone: for such a composition must be made by rays that have only two degrees of refrangibility, by Def. I. and III; and therefore it cannot be like that of the sun's light, by Prop. I; nor, for the same reason, like that of ordinary White objects.

V. Whiteness, in all respects like that of the sun's immediate light, cannot be compounded of simple colours, without an indefinite variety of them: for to such a composition there are requisite rays endued with all the indefinite degrees of refrangibility, by Prop. I. And those infer as many simple colours, by Def. I. and III. and Prop. II. and III.

To make these a little plainer, I have added also the Propositions that follow.

VI. The rays of light do not act on one another in passing through the same medium. This appears by several former passages, and is capable of further proof.

VII. The rays of light suffer not any change of their qualities from refraction.

VIII. Nor afterwards from the adjacent quiet medium. These two Propositions are manifest *de facto* in homogeneal light, whose colour and refrangibility is not at all changeable, either by refraction, or by the contermination of a quiet medium. And as for heterogeneal light, it is but an aggregate of several sorts of homogeneal light; no one sort of which suffers any more alteration than if it were alone; because the rays act not on one another, by Prop. VI. and therefore the aggregate can suffer none. These two Propositions also might be further proved apart by experiments, too long to be here described.

IX. There can no homogeneal colours be educed out of light by refraction, which were not commixt in it before; because by Prop. VII. and VIII. refraction changeth not the qualities of the rays, but only separates those which have diverse qualities, by means of the different refrangibility.

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X. The

X. The sun's light is an aggregate of an indefinite variety of homogeneous colours; by Prop. I. III. and IX. And hence it is, that I call homogeneous colours also primitive or original.

And thus much concerning colours.

Monf. *H.* has thought fit to insinuate, that the aberration of rays, by their different refrangibility, is not so considerable a disadvantage in glasses, as I seemed to be willing to make men believe, when I propounded concave mirrors as the only hopes of perfecting telescopes. But if he please to take his pen, and compute the errors of a glass and speculum, that collect rays at equal distances, he will find how much he is mistaken; and that I have not been extravagant, as he imagines, in preferring reflexions. And as for what he says of the difficulty of the *praxis*, I know it is very difficult; and by those ways which he attempted it, I believe unpracticable. But there is a way insinuated in the Transactions, p. 3080, by which it is not improbable, but that as much may be done in large telescopes, as I have thereby done in short ones; but yet not without more than ordinary diligence and curiosity.

Pray with these notes return my thanks to Mr. *Hugens* for his book.

By a former letter of your's, I was a little dubious whether Mr. *Slusius* might not apprehend, by what you wrote to him concerning me, that I pretended to his method of drawing tangents, until I understood by Mr. *Collins* that you signified to him, that you thought it here of a later date. For it seems to me, that he was acquainted with it some years before he printed his *Mesolabum*, and consequently before I understood it. But if it had been otherwise, yet since he first imparted it to his friends and the world, it ought deservedly to be accounted his. As for the methods, they are the same; though I believe derived from different principles. But I know not whether his principles afford it so general as mine; which extends to equations affected with surd terms, without reducing them to another form. But, if you please, let this pass.

The incongruities you speak of, I pass by. But I must, as formerly, signify to you, that I intend to be no farther solicitous about

about matters of philosophy. And therefore I hope you will not take it ill, if you find me never doing any thing more in that kind: or rather that you will favour me in my determination, by preventing, so far as you can conveniently, any objections, or other philosophical letters, that may concern me. For your proffer about my quarterly payments, I thank you. But I would not have you trouble yourself to get them excused, if you have not done it already. And now being tired with this long letter, I must, in haste, write myself your's, &c.

VIII.

Mr. NEWTON'S answer to Monf. HUGENIUS'S letter of Jan. 14, 1671.

SIR,

IT seems to me, that M. *Hugens* takes an improper way of examining the nature of colours, whilst he proceeds upon compounding those that are already compounded, as he doth in the former part of his letter. Perhaps he would sooner satisfy himself, by resolving light into colours, as far as may be done by art, and then by examining the properties of those colours apart; and afterwards by trying the effects of rejoining two or more, or all of those: and, lastly, by separating them again, to examine what changes that rejoinement had wrought in them. This will prove a tedious and difficult task, to do it as it ought to be done; but I could not be satisfied, till I had gone through it. However, I only propound it, and leave every man to his own method.

As to the contents of his letter, I conceive my former answer to the *quare* about the number of colours is sufficient, which was to this effect. That all colours cannot practically be derived out of Yellow and Blue, and consequently that those hypotheses are false, which imply they may. If you ask, what colours cannot be derived out of Yellow and Blue, I answer, none of those which I defined to be original; and if he can shew by experiment how they may, I will acknowledge myself in an error.

Nor

Of Slusius's
method of
Tangents.

The fallacy of
Mr. Hugens's
method of
examining
Colours.

All colours
not to be de-
rived from
Yellow and
Blue.

Nor is it easier to frame an hypothesis, by assuming only two original colours, rather than an indefinite variety; unless it be easier to suppose, that there are but two figures, sizes, and degrees of velocity, or force of the æthereal corpuscles or pulses, rather than an indefinite variety, which certainly would be a very harsh supposition. No man wonders at the indefinite variety of waves of the sea, or of sands on the shore; but were they all of but two sizes, it would be a very puzzling phenomenon. And I should think it as unaccountable, if the several parts or corpuscles of which a shining body consists, which must be supposed of various figures, sizes, and motions, should impress but two sorts of motion on the adjacent æthereal medium; or any other way beget but two sorts of rays. But to examine how colours may be thus explained hypothetically, is, besides, my purpose. I never intended to shew wherein consists the nature and difference of colours, but only to shew, that *de facto* they are original and immutable qualities of the rays which exhibit them; and to leave it to others to explicate, by mechanical hypotheses, the nature and difference of the qualities; which I take to be no very difficult matter. But I would not be understood, as if their difference consisted in the different refrangibility of those rays; for that different refrangibility conduces to their production no otherwise, than by separating the rays whose qualities they are: Whence it is, that the same rays exhibit the same colours, when separated by any other means; as by their different reflexibility; a quality not yet discoursed of.

White not to
be produced
from Yellow,
Green and
Blue.

In the next particular, where M. *Hugens* would shew, that it is not necessary to mix all colours for the production of White; the mixture of Yellow, Green, and Blue, without Red or Violet; which he propounds for that end, will not produce White, but Green; and the brightest part of the Yellow will afford no other colour but Yellow, if the experiment be made in a room well darkened, as it ought, because the coloured light is much weakened by the reflexion, and so apt to be diluted by the mixing of the other scattering light. But yet there is an experiment or two mentioned in my letter in the Transactions, Numb. 88, by which I have produced White out of two colours alone, and that vari-

ously,

ously, as out of Orange and a full Blue, and out of Red and pale Blue, and out of Yellow and Violet; as also out of other pairs of intermediate colours. The most convenient experiment for performing this was, that of casting the colours of one prism upon those of another, after a due manner. But what M. *Hugens* can deduce from hence, I see not; for the two colours were compounded of all others, and so the resulting White, to speak properly, was compounded of them all, and only decomposed of those two. For instance, the Orange was compounded of Red, Orange, Yellow, and some Green; and the Blue and Violet, full Blue, light Blue, and some Green, with all their intermediate degrees; and consequently the Orange and Blue together made an aggregate of all colours, to constitute the White. Thus if one mix Red, Orange and Yellow powders, to make an Orange; and Green, Blue, and Violet powders, to make a Blue: and, lastly, the two mixtures to make a Grey: that Grey, though decomposed of no more than two mixtures, is yet compounded of all the six powders as truly, as if the powders had been all mixed at once. This is so plain, that I conceive there can be no further scruple; especially to them who know how to examine whether a colour be simple or compounded, and of what colours it is compounded: which having explained in another place, I need not now repeat. If therefore M. *Hugens* would conclude any thing, he must shew how White may be produced out of two uncompounded colours; which, when he hath done, I will further tell him why he can conclude nothing from that. But I believe there cannot be found an experiment of that kind; because, as I remember, I once tried by gradual succession the mixture of all pairs of uncompounded colours, and though some of them were paler and nearer to White than others, yet none could be truly called White. But it being some years since this trial was made, I remember not well the circumstances; and therefore recommend it to others to be tried again.

In the last place, had I thought the distinctness of the picture which (for instance) a twelve-foot object-glass casts into a dark-
Of the aberrant rays in Telescopes.
ened room, to be so contrary to me as M. *Hugens* is pleased to affirm, I should have mended my Theory in that point, before I propounded

propounded it. For that I had thought on that difficulty, you may easily guess by an expression somewhere in my first letter to this purpose, that I wondered how telescopes could be brought to so great perfection by refractions, which were so irregular. But to take away the difficulty, I must acquaint you, first, That though I put the greatest lateral error of the rays from one another to be about $\frac{1}{50}$ of the glass's diameter, yet their greatest error from the points, on which they ought to fall, will be but $\frac{1}{100}$ of that diameter: and then, that the rays, whose error is so great, are but very few, in comparison to those which are refracted more justly. For the rays, which fall upon the middle parts of the glass, are refracted with sufficient exactness; as also are those that fall near the perimeter, and have a mean degree of refrangibility. So that there remain only the rays which fall near the perimeter, and are most or least refrangible, to cause any sensible confusion in the picture. And these are yet so much further weakened by the greater space, through which they are scattered; that the light which falls on the due point is infinitely more dense, than that which falls on any other point round about it. Which, though it may seem a paradox, yet is easily demonstrable. Yea, although the light, which passeth through the middle parts of the glass, were wholly intercepted, yet would the remaining light convene infinitely more dense at the due points, than at other places. And by this axis of density, the light which falls in, or insensibly near, the just point, may, I conceive, strike the sensorium so vigorously, that the impress of the weak light, which errs round about it, shall, in comparison, not be strong enough to be animadverted, or to cause any more sensible confusion in the picture, than is found by experience. This I conceive is enough to shew why the picture appears so distinct, notwithstanding the irregular refraction. But if this satisfy not, M. *Hu- gens* may try, if he please, how distinct the picture will appear, when all the lens is covered, excepting a little hole next its edge on one side only. And if in this case he please to measure the breadth of the colours, thus made at the edge of the sun's picture, he will perhaps find it approach nearer to my proportion than he expects.

X.

To Mr. OLDENBURG.

SIR,

WHEN you shewed me Mr. *Line's* second letter, I remember I told you, that I thought an answer in writing would be insignificant; because the dispute was not about any ratiocination, but my veracity in relating an experiment, which he denies will succeed, as it is described in my printed letter: for this is to be decided not by discourse, but new trial of the experiment.

What it is that imposes upon Mr. *Line*, I cannot imagine; but I suspect he has not tried the experiment since he acquainted himself with my Theory; but depends upon his old notions, taken up before he had any hint given to observe the figure of the coloured image. I shall desire him, therefore, before he returns any answer, to try it once more for his satisfaction, and according to this manner.

Let him take any prism, and hold it so that its axis may be perpendicular to the sun's rays; and in this posture let it be placed as close as may be to the hole, through which the sun shines into a dark room; which hole may be about the bigness of a pea. Then let him turn the prism slowly about its axis, and he shall see the colours move upon the opposite wall; first, towards that place to which the sun's direct light would pass, if the prism were taken away; and then back again. When they are in the middle of these two contrary motions, that is, when they are nearest that place to which the sun's direct ray tends, there let him stop; for then are the rays equally refracted on both sides the prism. In this posture of this prism, let him observe the figure of the colours; and he shall find it not round, as he contends, but oblong; and so much the more oblong as the angle of the prism, comprehended by the refracting planes, is bigger; and the wall, on which the colours are cast, more distant from the prism; the colours, Red, Yellow, Green, Blue, Purple, succeeding in order, not from one side of the figure to the other, as in Mr. *Line's* conjecture,

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Directions for
making the
experiment
with the
Prism.

jection, but from one end to the other; and the length of the figure being not parallel, but transverse to the axis of the prism. After this manner I used to try the experiment; and it will not succeed well, if the day be not clear, and the prism placed close to the hole; or so near at least, that all the sun's light, that comes from the hole, may pass through the prism also, so as to appear in a round form, if intercepted by a paper immediately after it has passed the prism.

When Mr. *Line* has tried this, I could wish he would proceed a little further, to try that which I called the *experimentum crucis*. For when he has tried them (which, by his denying them, I know he has not done yet as they should be tried) I presume he will rest satisfied.

Three or four days after you gave me a sight of Mr. *Line's* second letter, I remember I thereupon shewed the first of these two experiments to that gentleman, whom you found with me when you gave me that visit. And whilst I was shewing it to him, *A. H.* a member of the Royal Society, came in; and I shewed it to him also. And you may remember, that *R. H.* two or three years ago, in a letter read before the Royal Society, and transmitted to me, gave testimony not only to the experiments questioned by Mr. *Line*, but to all those set down in my first letter about colours, as having tried them himself: and when you read Mr. *Line's* letter at a meeting of the said Society, and was pleased to do me the favour to propound the experiment to be tried in their presence, *R. H.* spoke of it to them as a thing not to be questioned. But if it have not yet been tried before them, and any of them, upon Mr. *Line's* confidence, doubt of it; I promise, when I shall have the happiness to be at any more of their assemblies, upon the least hint, to shew them the trial of it; and I hope I shall not be troublesome, because it may be tried, though not so perfectly, even without darkening a room, or the expence of any more time than half a quarter of an hour; although, if Mr. *Line* persist in his denial of it, I could wish it might be tried sooner there, than I shall have an opportunity to be among them.

I have

I have returned you Mr. *Line's* letter. It came to my hands but this week; the gentleman by whom you sent it having not yet been at *Cambridge*, but transmitting it to me from *Oxford*.

I had some thoughts of writing a further discourse about colours to be read at one of your Assemblies; but find it yet against the grain, to put pen to paper any more on that subject. But however, I have one discourse by me of that subject, written when I sent my first letters to you about colours, and of which I then gave you notice. This you may command, when you think it will be convenient, if the custom of reading weekly discourses still continue. In the mean while, I am, &c.

X.

To Mr. OLDENBURG.

SIR,

Jan. 30, 1678.

CONCERNING the experiment of the glass and papers, I should add these two to the former directions. One, that the glass be rubbed with a full handful of stuff, which may cover and rub all the glass at once: for thus its electric virtue will be more easily and vigorously excited, than if rubbed with a little only, doubled up but once or twice. This rubbing with the stuff, I suppose, rarifies and diffuses the electric *effluvia* from the glass into the air; and the knocking or rubbing with the finger-ends puts the diffused *effluvia* into irregular motions. The other thing I would note is, that the papers may perhaps be too little, as well as too great. Too small ones will be apter to stick to the glass or table. If the experiment be tried with a glass three or four inches broad, set about $\frac{1}{4}$ of an inch from the table, and the papers of a thin sort of paper cut into triangular pieces, the sides of those triangles may not unfitly be about the 20th or 25th part of an inch, more or less. It may be best tried with bits of several sizes put in at once; and if there be put in a piece or two of the wing of a fly, those, I find, will move more easily, though scarce so variously. These and the former directions observed, I cannot imagine how you should miss; though I cannot promise

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all things will appear justly to you as they do to me, there being unaccountable circumstances, which may make a difference.

Reply to insinuations of Mr. Hook's.

I am obliged to you, Sir, for your candour, in acquainting me with Mr. Hook's insinuations. It is but a reasonable piece of justice, I should have an opportunity to vindicate myself from what may be undeservedly cast on me; and therefore, since you have been pleased to be my representative there, and I have no means of knowing what is done but by you, I hope you will continue that equitable candour; though I think the present business of no great moment as to me, not imagining that the Royal Society are to be imposed on in a thing so plain, or that Mr. Hook himself will persist in a mistake, when he hears the difference stated. The only thing I said he could pretend taken from his hypothesis, was the disposition of æther to vibrate; and yet whilst he grasps at all, he is likely to fall short of this too. That æthereal vibrations are light, is his; but that æther may vibrate (which is all, I suppose) is to be had from a higher fountain: for that æther is a finer degree of air, and air a vibrating medium, are old notions, and the principles I go upon. I desire Mr. Hook to shew me, therefore, I say not only the sum of the hypothesis I wrote, which is his insinuation, but only part of it taken out of his *Micrographia*; but then I expect too, that he instance in what is his own. It is most likely he will pretend, I had from him the application of vibrations to the solution of the phenomena of thin plates: and yet all the use I make of vibrations, is, to strengthen or weaken the reflecting power of the æthereal superficies; which is so far from being in his *Micrographia*, that the last spring, when I told him of the reflecting power of the æthereal superficies, he took it for a new notion; having till then supposed light to be reflected by the parts of gross bodies. To the things that he has from *Des Cartes*, pray add this, that the parts of solid bodies have a vibrating motion; least he should say I had from him what I say about heat. And his having from *Des Cartes* the reduction of all colours to two, you may, if need be, explain further for me thus. That as *Des Cartes* puts every globulus to be urged forward on one side by the illuminated medium, and impeded on the other by the dark one; so Mr. Hook puts

puts every vibration to be promoted at one end, and retarded at the other by those mediums; and thence both alike derive two modifications of light, on the two sides of the refracted beam, for the production of all colours.

By Mr. Gascoin's letter, one might suspect that Mr. Linus tried the experiment some other way than I did; and therefore I shall expect, till his friends have tried it according to my late discoveries: in which trial it may possibly be a further guidance to them to acquaint them, that the prism casts from it several images. One is that oblong one of colours which I mean, and this is made by two refractions only. Another there is, made by two refractions and an intervening reflexion; and this is round and colourless, if the angles of the prism be exactly equal; but if the angles at the reflecting base be not equal, it will be coloured; and that so much the more, by how much unequaller the angles are; but yet not much unround, unless the angles be very unequal. A third image there is made by one simple reflexion; and this is always round and colourless. The only danger is in mistaking the second for the first. But they are disagreeable not only by the length and lively colours of the first, but by its different motion too: for whilst the prism is turned continually the same way about its axis, the second and third move swiftly, and go always on the same way till they disappear; but the first moves slow; and grows continually slower, till it be stationary; and then turns back again, and goes back faster and faster, till it vanish in the place where it began to appear. If without darkening their room, they hold the prism at their window in the sun's open light; in such a posture, that its axis be perpendicular to the sunbeams; and then turn it about its axis, they cannot miss of seeing the first image: which having found, they may double up a paper once or twice, and make a round hole in the middle of it about $\frac{1}{4}$ or $\frac{1}{2}$ of an inch broad, and hold the paper immediately before the prism, that the sun may shine on the prism through that hole; and the prism being stayed and held steady, in that posture which makes the image stationary; if the image then fall directly on an opposite wall, or on a sheet of paper placed at the wall, suppose 15 or 20 foot from the prism,

or

or further off, they will see that image in such an oblong figure as I have described, with the Red at one end, the Violet at the other, and a blueish Green in the middle; and if they obscure their room as much as they can by drawing curtains or otherwise, it will make the colours the more conspicuous.

This direction I have set down, that no body, into whose hands a prism shall happen, may find difficulty or trouble in trying it. But when Mr. *Linus's* friends have tried it thus, they may proceed to repeat it in a dark room, with a less hole made in their window-shut. And then I shall desire, that they will send you a full and clear description how they tried it, expressing the length, breadth, and angles of the prism; its position to the incident rays, and to the window-shut; the bigness of the hole in that shut, through which the sun shined on their prism; what side of the prism the sun shined on; and at what side the light came out of it again; the distance of the prism from the opposite paper or wall, on which the refracted light was cast perpendicularly; and the length, breadth, and figure of the space there illuminated by that light; and the situation of each colour within that figure: and if they please to illustrate their description with a scheme or two, it will make the business plainer. By this means, if there be any difference in our way of experimenting, I shall be the better enabled to discern it, and give them notice where the failure is, and how to rectify it. I should be glad too, if they would favour me with a description of the experiment, as it has been hitherto tried by Mr. *Linus*; that I may have an opportunity to consider, what there is in that which makes against me. And because Mr. *Gascoin* seems to suspect, that my directions, sent Mr. *Linus*, differ from what I have printed; I desire also that he would signify wherein he thinks they may differ, so as to need reconciling. Fuller they are, but not different, nor any other than I have followed above these seven years.

History of the
dispute with
Mr. *Line*.

As for my suspicion, that Mr. *Linus* might possibly rely on old experiments; his quoting Sir *Kenelm Digby* for a by-stander, might have made any other stranger to his way, as well as me, suspect it. But I wonder most at Mr. *Gascoin's* insinuation, as if I influenced the press in what concerns Mr. *Linus* and me. You know, Sir,

I never

I never spoke or hinted a syllable to you, concerning printing or not printing any thing of Mr. *Linus*; nor so much as knew of the printing his first letter, till it was out in the Transactions. When you sent it to me, I, out of a great desire to avoid controversies (which, as you know, I had entertained long before) wrote back to you, that I had no mind to meddle with it: but, as I was ready to seal that letter, I added a postscript to this purpose: That seeing Mr. *Linus* was designing something about light for the press, to prevent publishing his mistake, you might, if you thought fit, signify to him (but not from me) that the experiment was tried otherwise than he suggested; and that in such and such respects, which I there named. And the substance of this postscript was that you published at the end of his first letter, on which Mr. *Gascoin* here animadvert; but was so far from being designed for the press by me, that the first sight of it, together with his letter in the Transactions, made me say to one, that I wished they had been suppressed; for I doubted the printing them would make Mr. *Linus* unquiet, and so in the end create me trouble. As for his second letter which you shewed me at London, I returned it again to you so soon as I had read it, and never saw it since, persisting in my desire to avoid the controversy. And at my returning it, you moved me for an answer, with this argument, that if I waved it, Mr. *Linus* was like to make the more stir: to which I replied, that the business, being about matter of fact, was not proper to be decided by writing, but by trying it before competent witnesses. Whereupon, at your motion, I told you what was requisite; and by your procurements, preparations were accordingly made for its trial at the next assembly of the Royal Society, as I understood by Mr. *Hook*: but the day proved cloudy, and before another assembly I returned to Cambridge, and from that time never enquired after nor regarded the matter further, till you sent me Mr. *Linus's* third letter. This is the history of Mr. *Linus's* business, so far as I know it: which I have set down, that his friends may see he has not been dealt with obliquely, as they seem to apprehend. All, I think, that they can object to you is, that you were at a stand, because you could not engage me in the controversy; and to me, that I had no mind

mind to be engaged: a liberty every body has a right to, and may gladly make use of sometimes at least, and especially if he want leisure, or meet with prejudice or groundless insinuations. But I hope to find none of this in Mr. *Gascoine*. The handsome genius of his present letter, makes me hope it for the future. In the mean time I desire with him, that you would publish Mr. *Linus's* letters as soon as you can conveniently, to prevent further misapprehensions. I am, &c.

XI.

To Mr. OLDENBURG.

SIR,

Cambridge,
Feb. 29, 1678.

BY reading Mr. *Linus's* letter, when you shewed it to me at *London*, I retained only a general remembrance, that Mr. *Linus* denied what I affirmed, and so could say nothing in particular to it. But having the opportunity to read it again in Numb. cxxi. of the *Transactions*, I perceive he would persuade you, that the information you gave him about the experiment is as inconsistent with my printed letters, as with experience. And therefore, lest any, who have not read those letters, should take my silence in this point for an acknowledgment, I thought it not amiss to send you something in answer to this also.

He tells you, that "whereas you assure him, First, that the experiment was made in clear days; secondly, that the prism was placed close to the hole, so that the light had no room to diverge; and, thirdly, that the image was not parallel, but transverse to the axis of the prism: if these assertions be compared with my relation of the experiment in the *Phil. Trans.* Numb. Lxxx. p. 3076. it will evidently appear they cannot be admitted, as being directly contrary to what is there delivered." His reasons are these:

First, that I said the ends of the long image seemed semicircular; which, says he, "never happens in any of the three cases above-said." But this is not to set me at odds with myself, but with the experiment. For it is there described to happen in them all;

all; and I still say it doth happen in them. Let others try the experiment, and judge.

Further he says, that the prism is placed at a distance from the hole in the scheme of the experiment in Numb. Lxxxiv. p. 4091^(a). But what if it were so there? For that is the scheme of a demonstration, not of the experiment; and would have served for the demonstration, had the distance been put twenty times greater than it is. In the schemes of the experiment, Numb. Lxxx. p. 3086^(b), and Numb. Lxxxiii. p. 4061^(c), it is represented close, and close enough the scheme, Numb. Lxxxiii. p. 4061^(c). But Mr. *Linus* thought fit to wink at those, and pitch upon the scheme of a demonstration; and such a scheme too, as hath no hole at all represented in it. For the scheme, Numb. Lxxxiv. p. 4091, is this^(a). In which the rays are not so far distant from one another at G, L; but that the hole, had I expressed it, might have been put there, and yet have comprehended them. But if we should put the hole at x, their decussation, yet it will not be any thing to his purpose; the distance xo, or xL, being but about half the breadth of a side of the prism ($\frac{1}{2}ac$); which I conceive is not a twentieth part of the distance requisite in his conjecture.

Thirdly, he says, that "more might be said out of my relation to shew, that the image was not transverse; for if it had been transverse, I could not have been surprized (as I said I was) to see the length thereof so much exceed the breadth; it being a thing so obvious and easy to be explicated by the ordinary rules of refraction." But, on the contrary, it may rather be said, that if the image had been parallel, I could not have been surprized to see the length thereof so much exceed the breadth, it being a thing so extremely obvious, as not to need any explication. For who, that had but common sense, and saw the whole prism, or a good part of it illuminated, could not expect that the light should have the same long figure upon the wall, that it had when it came out of the prism? Mr. *Linus*, therefore, while he would strengthen his argument, by representing me well skilled in optics, does but overthrow it. But where he says, "I could not have been surprized at the length, had the image been transverse, it being a thing so obvious, and easy to

^(a) See p. 308, lin. 20.^(b) See p. 306, lin. 30.^(c) See p. 312.

"be explicated by the ordinary rules of refraction." Let any man take the experiment entire, as I have there delivered it, that is with this condition, that the refractions on both sides the prism were equal, and try if he can reconcile it with the ordinary rules of refraction. On the contrary, he may find the impossibility of such a reconciliation demonstrated, in my answer to *P. Pardies*. Num. LXXXIV. p. 4091^(*).

In the last place he objects, that my saying, in Num. LXXX. p. 3077^(b), that the incident refractions were, in the experiment, equal to the emergent, proves again that the long image was parallel. And yet that very saying is a sufficient argument that I meant the contrary; because it becomes wholly impertinent, if applied to a parallel image; but in the other case is a very necessary circumstance. What is added, therefore, of *P. Pardies*, might have been spared; especially since that learned person understood my discourse to be meant of a transverse image, and acquiesced in my answers.

This in answer to Mr. *Linus's* letter. And now to take away the like suspicion from his friends, if my declaration of my meaning satisfy not; I shall note some further passages in my letters, whereby they may see how I was to be understood from the beginning, as to the aforesaid circumstances.

For the day; I express every where that the experiment was tried in the sun's light; and in Num. LXXX. p. 3077^(c), that the breadth of the image by measure answered to the sun's diameter. But because it is pretended I was imposed upon; I would ask, what the experiment, as it is advanced to that which I called the *experimentum crucis*, can have to do with a cloudy day. For if the *experimentum crucis*, which is that which I depend on, can have nothing to do with a cloudy day, then it is to no purpose to talk of a cloudy day in the first experiment, which does but lead on to that. But if this satisfy not, let the Transactions, in Num. LXXXIII. p. 4060, be consulted^(d). For there I tell you, how, by applying a lens to the prism, the straight edges of the oblong image became distincter than they would have been without the lens: a circumstance which cannot happen in Mr. *Linus's* case of a bright cloud.

(*) See p. 308. (b) See p. 297. (c) See p. 297, lin. 4 and 5. (d) See p. 311, lin. 25—27.

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For the position of the prism; I tell you, Num. LXXX. p. 3076^(*), that it was placed at the sun's entrance into the chamber: and in page 3085^(b), I had to make a hole in the shut, and there place the prism: and in the next page I say again, that the prism ABC is to be set close by the hole F of the window EG; and accordingly represent it close in the figure. Also in page 3077^(c) I tell you, that the distance of the image from the hole, or prism, was 22 feet: which is as much as to say, that the prism, suppose that side of it next the hole, was as far from the image as the hole itself was; and consequently that the prism and the hole were contiguous. Also in p. 3078^(d), where instead of the window-shut I made use of a hole in a loose board, I tell you expressly, that I placed the board behind the prism. All these passages are in my very first letter about colours. And who, therefore, would imagine, that any one, that had read that letter, should so much as suspect, that I placed the prism, I say not at so great a distance as Mr. *Linus* supposes, but at any distance worth considering.

Lastly, for the position of the image, it is represented transverse to the axis of the prism in the figures Num. LXXX. p. 3086^(e), Num. LXXXIII. p. 4061^(f), and Num. LXXXV. p. 5016^(g). And in Num. LXXXVIII. p. 5093^(h), where I made use of two cross prisms, I tell you expressly, that the image was cross to both of them, at an angle of 45 degrees. The calculation also, Num. LXXX. p. 3077⁽ⁱ⁾, are not to be understood without supposing the image cross. Nor are my notions about different refrangibility otherwise intelligible. For in Mr. *Linus's* supposition, the rays that go to the two ends of the image are equally refracted. So for colours: The Red, according to my description, falls at one end of the image; and the Blue, at the other; which cannot happen but in a transverse image. The same position is also demonstrable from what I said in Num. LXXX. p. 3076^(k), about turning the long image into a round one, by the contrary refraction of a second prism, further explained in Num. LXXXIII. p. 4061^(l). For this is not to be done in Mr. *Linus's* furnace

(*) See p. 295, lin. 10.

(b) See p. 298, lin. 18.

(c) See p. 330, lin. 29.

(d) See p. 306, lin. 5—8.

(e) See p. 306, lin. 30.

(f) See p. 296.

(g) See p. 296, lin. 20.

(h) See p. 296, lin. 28.

(i) See p. 312.

(k) See p. 317.

(l) See p. 312.

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of a parallel image; and therefore had Mr. *Linus* considered it, he could never have run into that surmise.

This I suppose is enough to manifest the three particulars: any one of which, being evidenced, is sufficient to take away the scruple. And therefore Mr. *Linus's* friends need not fear, but that the further directions I sent them lately for trying the experiment, are the same with those I have followed from the beginning; nor trouble themselves about any thing but to try the experiment right. But yet, because Mr. *Gascoin* has been pleased to insinuate his suspicion, that I do differ from himself in those directions; I shall not scruple here to reduce them into particulars, and shew where each particular is to be found.

First, then, he is to get a prism with an angle about 60 or 65 degrees (Num. LXXX. p. 3077 (*), and p. 3086 (b)). If the angle be about 63 degrees, as that was which I made use of (Num. LXXX. p. 3077 (*)), he will find all things succeed exactly as I described them there. But if it be bigger or less, as 30, 40, 50, or 70 degrees, the refraction will be accordingly bigger or less; and consequently the image longer or shorter. If his prism be pretty nearly equilateral, such as I suppose are usually sold in other places as well as in *England*, he may make use of the biggest angle. But he must be sure to place the prism so, that the refraction be made by the two planes which comprehend this angle. I could almost suspect, by considering some circumstances in Mr. *Linus's* letter, that his error was in this point; he expecting the image should become as long by a little refraction, as by a great one; which yet being too gross an error to be suspected of any optician, I say nothing of it; but only hint this to Mr. *Gascoin*, that he may examine all things.

Secondly, having such a prism, he must place it so, that the axis be perpendicular to the rays (Num. LXXXIV. p. 4091, lin. 18, 19 (c)). A little error in this point makes no sensible variation of the effect.

Thirdly, the prism must be so placed, that the refractions on both sides be equal (Num. LXXX. p. 3077 (d)), which shew it was

(*) See p. 296, lin. 33.

(b) See p. 306, lin. 3.

(c) See p. 308, lin. 20—22.

(d) See p. 296, lin. 33—35.

to be readily done, by turning it about its axis, and staying it when you let the image rest between two contrary motions, as I explained in my late descriptions, so I hinted before (Num. LXXX. p. 3077, lin. 34, 35, 36 (*)). If there should be an error in this point also, it can do no hurt.

Fourthly, the diameter of the hole I put $\frac{1}{4}$ of an inch (Num. LXXX. p. 3077 (b)), and placed the prism close to it, even so close as to be contiguous (Num. LXXX. p. 3077, lin. 4, 5 (c)). But yet there needs no curiosity in these circumstances. The hole may be of any other bigness, and the prism at a distance from the hole; provided things be so ordered, that the light appear of a round form, if intercepted perpendicularly at its coming out of the prism. Nor needs there any curiosity in the day. The clearer it is, the better; but if it be a little cloudy, that cannot much prejudice the experiment, so the sun do but shine distinctly through the cloud.

These things being thus ordered, if the refracted light fall perpendicularly on a wall or paper, at 20 feet or more from the prism; it will appear in an oblong form, cross to the axis of the prism, Red at one end, and Violet at the other; the length five times the breadth, more or less, according to the quantity of the refraction; the sides, straight lines parallel to one another; and the ends confused, but yet seeming semicircular.

I hope, therefore, Mr. *Linus's* friends will not entertain themselves any further about incongruous surmises, but try the experiment, as Mr. *Gascoin* has promised. And then, since Mr. *Gascoin* tells you, that “the experiment being of itself extraordinary and surprizing, and besides ushering in new principles into optics, quite contrary to the common and received; it will be hard to persuade it at a truth, till it made so visible to all, as it were a shame to deny it;” if he esteem it so extraordinary, he may have the privilege of making it so visible to all, that it will be a shame to deny it. For I dare say, after his testimony, nobody else will scruple it. And I make no question but he will hit of it, it being so plain and easy, that I am very much at a loss to imagine what way Mr. *Linus* took to miss.

(*) See p. 297, lin. 20—23.

(b) See p. 296, lin. 29.

(c) See p. 296, lin. 28.

XII.

To Mr. OLDENBURG.

SIR,

THE things oppos'd by Mr. *Line* being upon trials found true and granted me, I begin with the new question about the proportion of the length of the image to its breadth. I call it a new one. For though Mr. *Line* in his last letter spake against so great a length, as I assign; yet, as it seems to me, it was not to grant any transverse length shorter than that assigned by me (for in his first letter he absolutely denied that there would be any such length) but to lay the greater emphasis upon his discourse, whilst in defence of common optics, he was disputing in general against a transverse image. And therefore, in my answer, I did not prescribe the just quantity of the refracting angle, with which I would have the experiment repeated: which would have been a necessary circumstance, had the dispute been about the just proportion of the length to the breadth. Yet I added * this note,

* In my first letter in Phil. Transf. Num. cxxi. p. 500.

that the bigger the angle of the prism is, the greater will be the length in proportion to the breadth (*); not imagining, but that when he had found in any prism the length of the image transverse to the axis, he would easily thence conclude, that a prism with a greater angle would make the image longer; and consequently, that by using an angle great enough, he might bring it to equal, or exceed the length assigned by me; as indeed he might: for, by taking an angle of 70 or 75 degrees, or a little greater, he might have made the length not only five, but six or eight times the breadth, or more. No wonder, therefore, that Mr. *Lucas* found the image shorter than I did, seeing he tried the experiment with a less angle.

The angle, indeed, which I used was but about 63 deg. 12 min. and his is set to 60 degrees; the difference of which from mine being but 3 deg. 12 min. is too little to reconcile us; but yet it will bring us considerably nearer together. And if this angle was not exactly measured, but the round number of 60 degrees set down by guess, or by a less accurate measure (as I sus-

(*) See p. 353, lin. 30, 31

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pect by the conjectural measure of the refraction of his prism, by the ratio of the sines as 2 to 3, set down at the same time, instead of an experimental one), then might it be 2 or 3 degrees less than 60 deg. if not still less: and all this, if it should be so, would take away the greatest part of the difference between us.

But however it be, I am well assured my own observation was exact enough. For I have repeated it divers times since the receipt of Mr. *Lucas's* letter, and that without any considerable difference of my observations, either from one another, or from what I wrote before: and that it might appear experimentally, how the increase of the angle increases the length of the image, and also that nobody, who has a mind to try the experiment exactly, might be troubled to procure a prism which has an angle just of the bigness assigned by me, I tried the experiment with divers angles, and have set down my trials in the following Table; where the first column expresses the angles of two prisms which I used, which are measured as exactly as I could, by applying them to the angle of a sector; and the second column expresses, in inches, the length of the image made by each of those angles; its breadth being two inches, its distance from the prism 18 feet and 4 inches, and the breadth of the hole in the window-shut $\frac{1}{4}$ of an inch.

Angles.		Lengths.		Angles.		Lengths.	
The first prism	56° 10'	7 $\frac{1}{4}$		The second prism	54° 0'	7 $\frac{1}{2}$	
	60 24				62 12		
	63 26				63 48		
		9 $\frac{1}{2}$				10 $\frac{1}{2}$	
		10 $\frac{1}{4}$				10 $\frac{1}{4}$	

You may perceive, that the length of the images, in respect of the angles that made them, are something greater in the second prism than in the first; but that was because the glass, of which the second prism was made, had the greater refractive power.

The days in which I made these trials were pretty clear, but not so clear as I desired; and therefore, afterwards meeting with a day as clear as I desired, I repeated the experiment with the second prism, and found the lengths of the image made by its several angles, to be about $\frac{1}{4}$ of an inch greater than before; the measures being those set down in the Table.

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THEORY OF

	Angles.	Lengths.
The second prism	54° 0'	7 $\frac{1}{3}$
	62 12	10
	63 48	11

The reason of this difference, I apprehend, was, that in the clearest days the light of the white skies, which dilutes and renders invisible the faintest colours at the ends of the image, is a little diminished in a clear day, and so gives leave to the colours to appear to a great length; the sun's light at the same time becoming brisker, and so strengthening the colours, and making the faint ones at the two ends more conspicuous: for I have observed, that in days something cloudy, whilst the prism has stood unmoved at the window, the image would grow a little longer or a little shorter, accordingly as the sun was more or less obscured by thin clouds, which passed over it; the image being shortest while the cloud was brightest, and the sun's light faintest. Whence it is easy to apprehend, that if the light of the clouds could be quite taken away, so that the sun might appear surrounded with darkness, or if the sun's light were much stronger than it is, the colours would still appear to a greater length.

In all these observations the breadth of the image was just two inches. But observing that the sides of the two prisms I used were not exactly plain, but a little convex (the convexity being about so much as that of a double convex-glass of a sixteen or eighteen-foot telescope) I took a third prism, whose sides were as much concave as those of the other were convex: and this made the breadth of the image to be two inches and a third part of an inch; the angles of this prism, and the lengths of the image made by each of those angles, being those expressed in this Table.

Angles.	Lengths.
58°	8 $\frac{1}{2}$
59 $\frac{1}{2}$	9
62 $\frac{1}{2}$	10 $\frac{1}{3}$

In this case you see the concave figure of the sides of the prism by making the rays diverge a little, causes the breadth of

LIGHT AND COLOURS.

the image to be greater in proportion to its length than it would be otherwise. And this I thought fit to give you notice of, that Mr. *Lucas* may examine, whether his prism hath not this fault. If a prism may be had with sides exactly plain, it may do well to try the experiment with that; but it is better if the sides be about so much convex as those of mine are, because the image will thereby become much better defined: for this convexity of the sides does the same effect, as if you should use a prism with sides exactly plain, and between it and the hole in the window-shut, place an object-glass of an 18 foot telescope, to make the round image of the sun appear distinctly defined on the wall, when the prism is taken away, and consequently the long image made by the prism to be much more distinctly defined, especially at its straight sides, than it would be otherwise.

One thing more I shall add: That the utmost length of the image, from the faintest Red at one end, to the faintest Blue at the other, must be measured. For in my first letter about colours, where I set down the length to be five times the breadth, I called that length the utmost length of the image; and I measured the utmost length, because I account all that length to be caused by the immediate light of the sun; seeing the colours, as I noted above, become visible to the greatest length in the clearest days, that is, when the light of the sun transcends most the light of the clouds. Sometimes there will happen to shoot out from both ends of the image a glaring light a good way beyond these colours; but this is not to be regarded, as not appertaining to the image. If the measures be taken right, the whole length will exceed the length of the straight sides by about the breadth of the image.

By these things set down thus circumstantially, I presume Mr. *Lucas* will be enabled to accord his trials of the experiment with mine; so nearly at least, that there shall not remain any very considerable difference between us. For if some little difference should still remain, that need not trouble us any further, seeing there may be many various circumstances which may conduce to it; such as are not only the different figures of prisms, but also the different refractive power of glasses, the different diameters

of the sun at divers times of the year, and the little errors that may happen in measuring lines and angles, or in placing the prism at the window; though, for my part, I took care to do these things as exactly as I could. However, Mr. *Lucas* may make sure to find the image as long or longer than I have set down, if he take a prism whose sides are not hollow ground, but plain, or (which is better) a very little convex, and whose refracting angle is as much greater than that I used, as that he hath hitherto tried it with is less; that is, whose angle is about 66 or 67 degrees, or, if he will, a little greater.

Concerning Mr. *Lucas's* other experiments, I am much obliged to him that he would take these things so far into consideration, and be at so much pains for examining them; and I thank him so much the more, because he is the first that hath sent me an experimental examination of them. But yet it will conduce to his more speedy and full satisfaction, if he a little change the method which he has propounded, and, instead of a multitude of things, try only the *Experimentum Crucis*: for it is not number of experiments, but weight to be regarded; and where one will do, what need many? Had I thought more requisite, I could have added more. For before I wrote my first letter to you about colours; I had then taken much pains in trying experiments about them, and written a tractate on that subject; wherein I had set down at large the principal of the experiments I had tried: amongst which there happened to be the principal of those experiments which Mr. *Lucas* has now sent me. And as for the experiments set down in my first letter to you, they were only such as I thought convenient to select out of that tractate.

But suppose those had been my whole store; yet Mr. *Lucas* should not have grounded his discourse upon a supposition of any want of experiment, till he had examined those few. For if any of those be demonstrative, they will need no assistants, nor leave room for further disputing about what they demonstrate.

The main thing he goes about to examine is, the different refrangibility of Light; and this I demonstrated by the *Experimentum Crucis*. Now if this demonstration be good, there needs no further

ther examination of the thing; if not good, the fault of it is to be shewn: for the only way to examine a demonstrated proposition is to examine the demonstration. Let that experiment therefore be determined in the first place, and that which it proves be acknowledged; and then, if Mr. *Lucas* wants my assistance to unfold the difficulties which he fancies to be in the experiments he has propounded, he shall freely have it. At present I shall say nothing in answer to his experimental discourse, but this in general; That it has proceeded partly from some misunderstanding of what he writes against, and partly for want of due caution in trying experiments; and that amongst his experiments, there is one, which, when duly tried, is, next to the *Experimentum Crucis*, the most conspicuous experiment, I know, for proving the different refrangibility of light, which he brings it to prove against.

By the postscript of Mr. *Lucas's* letter, one not acquainted with what has passed, might think that he quotes the observation of the Royal Society against me; whereas the relation of their observation, which you sent to *Liege*, contained nothing at all about the just proportion of the length of the image to its breadth according to the angle of the prism, nor any thing more (so far as I can perceive by your last) than what was pertinent to the things then in dispute, viz. that they found them succeed as I had affirmed. And therefore, since Mr. *Lucas* has found the same success, I suppose, that when he expresseth, That *he much rejoiced to see the trials of the R. Society agree so exactly with his*, he meant only so far as his agreed with mine.

And because I am again upon this first experiment, I shall desire that Mr. *Lucas* will repeat it with all the exactness and caution that may be, regard being had to the information about it, set down in this letter. And then I desire to have the length and breadth of the image, with its distance from the prism, set down exactly in feet and inches, and parts of an inch; that I may have an opportunity to consider what relation its length and breadth have to the sun's diameter. For I know that Mr. *Lucas's* observation cannot hold, where the refracting angle of the prism is full 60 degrees, and the day is clear, and the full length of the

colours is measured, and the breadth of the image answers to the sun's diameter. And seeing I am well assured of the truth and exactness of my own observations, I shall be unwilling to be diverted by any other experiments, to have a fair end made of this in the first place.

P. S. I had like to have forgotten to advise, that the *Experimentum Crucis*, and such others as shall be made for knowing the nature of colours, be made with prisms that refract so much, as to make the length of the image five times its breadth, and rather more than less; for otherwise, experiments will not succeed so plainly with others, as they have done with me:

L E T T E R S

RELATING TO THE

EXCITATION OF ELECTRICITY IN GLASS.

I.
EXTRACT FROM THE MINUTES
OF THE
ROYAL SOCIETY.

Dec. 9, 1675.

THE company taking particular notice, among other things, of an experiment mentioned in this hypothesis, desired it might be tried, *viz.* That having laid upon a table a round piece of glass, about two inches broad; in a brass ring, so that the glass might be about one-third of an inch from the table, and the air between them inclosed on all sides after the manner as if he had whelved a little sieve upon the table: and then rubbing the glass briskly, till some little fragments of paper, laid on the table under the glass, began to be attracted, and move nimbly to and fro; after he had done rubbing the glass, the papers would continue a pretty while in various motions; sometimes leaping up to the glass, and resting there a-while; then leaping down and resting there, and then leaping up and down again: and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also leaping up in one arch, and down in another, divers times together, without sensible resting between; sometimes skip in a bow from one part of the glass to another, without touching the table; and sometimes hang by a corner, and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind; and be otherwise variously moved, every paper with a divers motion. And upon sliding his finger on the upper side of the glass, though neither the glass nor inclosed air below were moved thereby, yet would the papers, as they hung under the glass, receive some new motion.

motion, inclining this or that way, according as he moved his finger.

The experiment he proposes to be varied with a larger glass placed further from the table, and to make use of bits of leaf-gold instead of papers, esteeming that this will succeed much better, so as perhaps to make the gold rise and fall in spiral lines, or whirl for a time in the air, without touching the table of glass.

Ordered, That this experiment be tried the next meeting.

II.

To Mr. OLDENBURG.

SIR,

Cambridge,
Dec. 14, 1675.

THE notice you gave me of the Royal Society's intending to see the experiment of glass rubbed, to cause various motions in bits of paper underneath, put me upon recollecting myself a little further about it. And then remembering, that if one edge of the brass hoop was laid downward, the glass was as near again to the table, as it was when the other edge was laid downward; and that the papers played best when the glass was nearest to the table; I began to suspect, that I had set down a greater distance of the glass from the table, than I should have done. For in setting down that experiment, I trusted to the idea I had of the bigness of the hoop; in which I might easily be mistaken, having not seen it of a long time. And this suspicion was increased by trying the experiment with an object-glass of a telescope placed about the third part of an inch from the table. For I could not see the papers play any thing near so well, as I had seen them formerly. Whereupon I looked for the old hoop with its glass; and at length found the hoop, the glass being gone. But by the hoop I perceived, that when one edge was turned down, the glass was almost the third part of an inch from the table: and when the other edge was down, which made the papers play so well, the glass was scarce the eighth part of an inch from the table. This I thought fit to signify to you, that if the experiment succeed not well at the distance I set down; it may be tried

at a less distance, and that you may alter my paper, and write in it an eighth part of an inch, instead of $\frac{1}{2}$ or $\frac{1}{3}$ of an inch. The bits of paper ought to be very little, and of thin paper; perhaps little bits of the wings of a fly, or other light substances, may do better than paper. Some of the motions, as that of hanging by a corner, and twirling about, and that of leaping from one part of the glass to another without touching the table, happen but seldom; but it made me take the more notice of them.

Pray present my humble service to Mr. Boyle when you see him, and thanks for the favour of the converse I had with him at Spring. My conceit at trepanning the common æther, as he was pleased to express it, makes me begin to have the better thoughts on, that he was pleased to entertain it with a smile. I am apt to think, that when he has a set of experiments to try in his air-pump, he will make that one to see how the compression or relaxation of a muscle will shrink or swell, soften or harden, lengthen or shorten it.

As for registering the two discourses, you may do it; only I desire you would suspend till my next letter, in which I intend to set down something to be altered, and something to be added in the hypothesis, being in the mean while, Sir, &c.

III.

Extract from the Minutes of the ROYAL SOCIETY.

Dec. 16, 1675.

MR. NEWTON'S experiment of glass rubbed to cause various motions in bits of paper underneath was tried, but succeeded not in these circumstances with which it was tried. This trial was made upon the reading of a letter of his dated the 14th of Dec. 1675, at Cambridge, wherein he gives some particular directions about the experiment.

Ordered, That the Secretary should write again to the said Mr. Newton, and acquaint him with the want of success of his experiment; and desire him, that he would send his own apparatus wherewith he had made it: as also to enquire whether he had

secured the papers, being moved by the air, that might somewhere steal in.

IV.

To Mr. OLDENBURG.

SIR,

Dec. 21, 1675.

UPON your letter, I took another glass, 4 inches broad and $\frac{1}{4}$ of an inch thick, of such glass as telescopes are made of; and placed it a $\frac{1}{4}$ part of an inch from the table. It was set in such a piece of wood, as the object-glasses of telescopes use to be set: and the experiment succeeded well. After the rubbing was still, and all was still, the motion of the papers would continue sometimes while I counted a hundred; every paper leaping up about twenty times more or less, and down as often. I tried it also with two other glass that belong to a telescope, and it succeeded with both. And I make no question but any glass will do, that shall be excited to electric virtue; as I think any may. If you have a mind to try any of these glasses, you may have them. But I suppose, if you cannot make it do in other glass, you will fail in any I can send you. I am apt to suspect the failure was in the manner of rubbing: for I have observed, that the rubbing variously, or with various things, alters the case. At one time I rubbed the aforesaid great glass with a napkin, twice as much as I used to do with my gown, and nothing would stir; and yet presently rubbing it with something else, the motions soon began. After the glass has been much rubbed too, the motions are not so lasting; and the next day I found the motions fainter and difficulter to excite than the first. If the Society have a mind to attempt it any more, I can give no better advice than this: To take a new glass not yet rubbed (perhaps one of the old ones may do well enough after it has lain still a while) and let this be rubbed, not with linen nor soft nappy woollen, but with stuff whose threads may rake the surface of the glass, suppose taffet-rine or the like doubled up in the hand; and this with a brisk motion as may be, till 100 or 150 may be counted; the glass lying

lying all the while over the papers. Then if nothing stir, rub the glass with the finger-ends half a score times to and fro, or knock your finger-ends as often upon the glass: for this rubbing or knocking with your finger-ends, after the former rubbing, conduces most to excite the papers. If nothing stir yet, rub again with the cloth till 60 or 80 may be counted; and then rub or knock again with your fingers, and repeat this till the electric virtue of the glass be so far excited as to take up the papers; and then a very little rubbing or knocking now and then will revive the motions. In doing all this, let the rubbing be always done as nimbly as may be; and if the motion be circular like that of glass-grinders, it may be better. But if you cannot make it yet succeed, it must be let alone till I have some opportunity of trying it before you. As for the suspicion of the paper being moved by the air, I am secure from that: yet in the other of drawing the leaf-gold to above a foot distance, which I never went about to try myself till the last week, I suspect the air might raise the gold, and then a small attraction might determine it towards the glass: for I could not make it succeed.

As for Mr. Hook's insinuation, that the sum of the hypothesis I sent you, had been delivered by him in his Micrography, I need not be much concerned at the liberty he takes in that kind. Yet because you think it may be well, if I state the difference I take to be between them, I shall do it as briefly as I can; and that the rather, that I may avoid the favour of having done any thing unjustifiable or unhandsome towards Mr. Hook. But for this end, I must first, to see what is his, cast out what he has borrowed from *Des Cartes*, or others, *viz.* That there is an æthereal medium: That light is the action of this medium: That this medium is less implicated in the parts of solid bodies, and so moves more freely in them, and transmits light more readily through them; and that after such a manner as to accelerate the rays in a certain proportion: That refraction arises from this acceleration, and has sines proportional: That light is at first uniform: That colours are some disturbance or modification of its rays by refraction, or reflexion: That the colours of a prism are made by means of the quiescent medium accelerating some motion of

the rays on one side, where Red appears, and retarding it on the other side, where Blue appears: and that there are but these two original colours or colour making modifications of light, which by their various degrees, or, as Mr. *Hook* calls it, dilutings, produce all intermediate ones. This rejected, the remainder of his hypothesis is, that he has changed *Des Cartes* pressing or progressive motion of the medium to a vibrating one; the rotation of the *globuli* to the obliquation of the pulses; and the accelerating their rotation on the one hand, and retarding it on the other by the quiescent medium, to produce colours, to the like action of the medium on the two ends of his pulse for the same end. And having thus far modified his by the Cartesian hypothesis, he has extended it farther to explicate the phenomena of thin plates; and added another explication of the colours of natural bodies fluid and solid.

This, I think, is in short the sum of his hypothesis. And in all this, I have nothing common with him, but the supposition that æther is a medium susceptible of vibrations. Of which supposition I make a very different use: he supposing it light itself; which I suppose it is not. This is as great a difference, as is between him and *Des Cartes*. But besides this, the manner of refraction and reflexion, and the nature and production of colours in all cases, which take up the body of my discourse, I explain very differently from him; and even in the colours of thin transparent substances, I explain every thing after a way so differing from him, that the experiments, I ground my discourse on, destroy all he has said about them. And the two main experiments, without which the manner of the production of those colours is not to be found out, were not only unknown to him when he wrote his *Micrography*, but even last Spring; as I understood in mentioning them to him. This therefore is the sum of what is common to us; That æther may vibrate. And so if he thinks fit to use that notion of colours arising from the various bigness of pulses, without which his hypothesis will do nothing, his will borrow as much from my answer to his objections, as that I send you does from his *Micrography*,

But it may be, he means, that I have made use of his observations. And of some I did: as that of the inflexion of rays, for which I quoted him: that of opacity arising from the interstices of the parts of bodies, which I insist not on: and that of plated bodies exhibiting colours; a phenomenon, for the notice of which I thank him. But he left me to find out and make such experiments about it, as might inform me of the manner of the production of those colours to ground an hypothesis on: he having given no farther insight into it than this, that the colour depended on some certain thickness of the plate. Though what that thickness was at every colour, he confesses in his *Micrography* he had attempted in vain to learn. And therefore seeing I was left to measure it myself, I suppose he will allow me to make use of what I took the pains to find out. And this I hope may vindicate me from what Mr. *Hook* has been pleased to charge me with.

Sir, I doubt I have already troubled you with too large a letter, and so break off abruptly, yours, &c.

V.

Extract from the Minutes of the ROYAL SOCIETY.

Dec. 30, 1675.

—READ a letter of Mr. *Newton's*, dated Dec. 21, 1675, in which he assures, that the experiment had been repeated by him again with good success, and gives yet further directions for the making of it successfully.

Ordered, That these directions should be observed at the next trial to be made at the next meeting of the Society.

VI.

Jan. 13, 1676.

MR. *NEWTON'S* experiment of glass rubbed to cause various motions in bits of paper underneath, being made according to his more particular directions, did succeed very well. The rubbing

ELECTRICITY.

rubbing was made both with a scrubbing-brush made of short hog's-bristles, with a knife, the haft of a knife made with whale-bone, and with the nail of one's finger. It appeared, that touching many parts at once with a hard and rough body, did produce the effect expected.

A

L E T T E R

TO THE

H O N. M R. B O Y L E

O N T H E

C A U S E O F G R A V I T A T I O N.

A L E T-

Copy of a Letter from SIR I. NEWTON

TO THE

HON. MR. BOYLE.

HON. SIR,

Feb. 28, 1678.

I HAVE so long deferred to send you my thoughts about the physical qualities we spoke of, that, did I not esteem myself obliged by promise, I think I should be ashamed to send them at all. The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself in them; and what I am not satisfied in, I can scarce esteem fit to be communicated to others, especially in natural philosophy, where there is no end of fancying. But because I am indebted to you, and yesterday met with a friend, Mr. *Mauhyverer*, who told me, he was going to *London*, and intended to give you the trouble of a visit, I could not forbear to take the opportunity of conveying this to you by him.

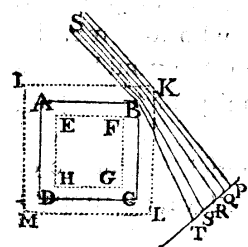
It being only an explication of qualities, which you desire of me; I shall set down my apprehensions in the form of suppositions, as follow.

And, first, I suppose that there is diffused through all places an æthereal substance, capable of contraction or dilatation, strongly elastic; and, in a word, much like air in all respects, but far more subtle.

2. I suppose this Æther pervades all gross bodies, but yet so as to stand rarer in their pores than in free spaces; and so much

the rarer, as their pores are less. And this I suppose (with others) to be the cause, why light, incident on those bodies, is refracted towards the perpendicular; why two well-polished metals cohere in a receiver exhausted of air: why quick-silver stands sometimes up to the top of a glass pipe, though much higher than 30 inches; and one of the main causes, why the parts of all bodies cohere; also the cause of philtration, and of the rising of water in small glass pipes, above the surface of the stagnating water they are dipped into: for I suspect the æther may stand rarer not only in the insensible pores of bodies, but even in the very sensible cavities of those pipes. And the same principle may cause menstruums to pervade with violence the pores of the bodies they dissolve; the surrounding æther as well as the atmosphere pressing them together.

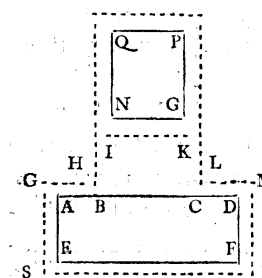
3. I suppose the rarer Æther within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another: the external æther beginning to grow rarer, and the internal to grow denser, at some little distance from the superficies of the body, and running through all intermediate degrees of density in the intermediate spaces. And this may be the cause why light, in *Grimaldi's* experiment, passing by the edge of a knife, or other opaque body, is turned aside, and as it were refracted; and by that refraction



makes several colours. Let ABCD be a dense body, whether opaque or transparent; EFGH, the outside of the uniform æther, which is within it; IKLM, the inside of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of density, between that of the two uniform æthers on either side. This being supposed, the rays of the sun, *ss*, *sk*, which pass by the edge of this body, between B and K, ought, in their passage through the unequally dense æther there, to receive a ply from the denser æther, which is on that side towards K; and that the more, by how much they pass nearer to the body; and thereby to be scattered through the space

space PQRT, as by experience they are found to be. Now the space between the limits EFGH and IKLM, I shall call the space of the æther's *graduated rarity*.

4. When two bodies, moving towards one another, come near together, I suppose the æther between them to grow rarer than before; and the spaces of its graduated rarity to extend further from the superficies of the bodies towards one another; and this by reason of the æther cannot move and play up and down so freely, in the strait passage between the bodies, as it would before they came so near together.



Thus, if the space of the æther's graduated rarity reach from the body ABCDEF, only to the distance GHLMS, when no other body is near it; yet may it reach farther, as to IK, when another body, NGPQ, approaches: and as the other body approaches more and more, I suppose the æther between them will grow rarer and rarer.

These suppositions I have so described, as, if I thought the spaces of gradual æther had precise limits, as is expressed at IKLM, in the first figure, and GMS, in the second: for thus I thought I could better express myself. But really, I do not think they have such precise limits, but rather decay insensibly; and, in so decaying, extend to a much greater distance, than can easily be believed, or need be supposed.

5. Now from the fourth supposition, it follows, that when two bodies, approaching one another, come so near together as to make the æther between them begin to rarify; they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another: which reluctance and endeavour will increase as they come nearer together, because thereby they cause the interjacent æther to rarify more and more: but at length, when they come so near together, that the excess of pressure of the external æther, which surrounds the bodies, above that of the rarified æther, which is between them, is so great, as to overcome the reluctance which the bodies have from

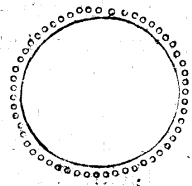
being brought together; then will that excess of pressure drive them with violence together, and make them adhere strongly to one another, as was said in the second supposition. For instance, in the second figure, when the bodies ED and NP are so near together, that the spaces of the æther's graduated rarity begin to reach to one another, and meet in the line IK; the other between them will have suffered much rarification, which rarification requires much force, that is, much pressing of the bodies together: and the endeavour, which the æther between them has to return to its former natural state of condensation, will cause the bodies to have an endeavour of receding from one another. But, on the other hand, to counterpoise this endeavour, there will not yet be any excess of density of the æther which surrounds the bodies, above that of the æther which is between them at the line IK. But if the bodies come nearer together, so as to make the æther in the mid-way-line, IK, grow rarer than the surrounding æther; there will arise, from the excess of density of the surrounding æther, a compression of the bodies towards one another; which, when by the nearer approach of the bodies, it becomes so great as to overcome the aforesaid endeavour the bodies have to recede from one other, they will then go towards one another, and adhere together. And, on the contrary, if any power force them asunder, to that distance where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now, hence I conceive it is, chiefly, that a fly walks on water without wetting her feet, and consequently without touching the water; that two polished pieces of glass are not without pressure brought to contact, no, not though the one be plain, the other a little convex; that the particles of dust cannot by pressing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging substances, and salts dissolved in water, do not of their own accord concrete and fall to the bottom, but diffuse themselves all over the liquor, and expand still more, if you add more liquor to them. Also, that the particles of vapours, exhalations, and air, do stand at a distance from one another, and endeavour to recede from one another, as the pressure of the incumbent atmosphere

atmosphere will let them: for I conceive the confused mass of vapours, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies, of which the earth consists, separated from one another, and kept at a distance by the said principle.

From these principles, the actions of menstruums upon bodies may be thus explained. Suppose any tinging body, as cochineal or logwood, be put into water, so soon as the water sinks into its pores, and wets on all sides, any particle, which adheres to the body, only by the principle in second supposition; it takes off, or at least much diminishes the efficacy of that principle, to hold the particle to the body, because it makes the æther on all sides the particle to be of a more uniform density than before. And then the particle being shaken off by any little motion, floats in the water, and with many such others, makes a tincture; which tincture will be of some lively colour, if the particles be all of the same size and density; otherwise of a dirty one. For the colours of all natural bodies whatever, seem to depend on nothing but the various sizes and densities of their particles: as I think you have seen described by me more at large in another paper. If the particles be very small (as are those of salts, vitriols, and gums) they are transparent; and as they are supposed bigger and bigger, they put on the several colours in order; Black, White, Yellow, Red; Violet, Blue, pale Green, Yellow, Orange, Red; Purple, Blue, Green, Yellow, Orange, Red, &c. as is discerned by the colours, which appear at the several thickneses of very thin plates of transparent bodies. Whence, to know the causes of the changes of colours, which are often made by the mixtures of several liquors, it is to be considered, how the particles of any tincture may have their size or density altered by the infusion of another liquor.

When any metal is put into common water, the water cannot enter into its pores to act on it and dissolve it: not that water consists of too gross parts for this purpose, but because it is unsociable to metal: for there is a certain secret principle in nature, by which liquors are sociable to some things, and unsociable to others. Thus water will not mix with oil, but readily with spirit

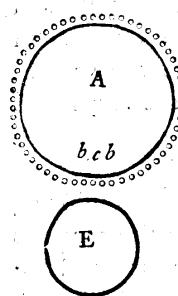
rit of wine, or with salts; it sinks also into wood, which quick-silver will not; but quick-silver sinks into metals, which water will not; so *aqua fortis* dissolves silver, not gold; *aqua regis* gold, and not silver, &c. But a liquor, which is of itself unfociable to a body, may, by the mixture of a convenient mediator, be made fociable: so molten lead, which alone will not mix with copper, or with *regulus of mars*, by the addition of tin, is made to mix with either: and water, by the mediation of saline spirits, will mix with metal. Now when any metal is put in water, impregnated with such spirits as into *aqua fortis*, *aqua regis*, *spirit of vitriol*, or the like; the particles of the spirits, as they in floating in the water strike on the metal, will, by their fociableness, enter into its pores, and gather round its outside particles, and, by advantage of the continual tremor the particles of the metal are in, hitch themselves in by degrees between those particles and the body, and loosen them from it; and the water entering into the pores together with the saline spirits, the particles of the metal will be thereby still more loosed, so as by that motion the solution puts them into to be easily shaken off, and made to float in the water: the saline particles still encompassing the metallic ones,



as a coat or shell does a kernell, after the manner expressed in the annexed figure; in which figure I have made the particles round; though they may be cubical, or of any other shape.

If into a solution of metal, thus made, be poured a liquor abounding with particles, to which the former saline particles are more fociable than to the particles of the metal (suppose with particles of salt of tartar); then, so soon as they strike on one another in the liquor, the saline will adhere to those particles more firmly than to the metalline ones; and by degrees be wrought off from those, to enclose these. Suppose A, a metalline particle enclosed with saline ones of spirit of nitre; and E, a particle of salt of tartar contiguous to two of the particles of the spirit of nitre *b* and *c*; and suppose the particle E is impelled by any motion towards *d*, so as to roll about the particle *c*, till it touch the particle *d*; the particle *b* adhering more firmly to E than to A, will be forced off from

A.



A. And by the same means the particle E, as it rolls about it, will tear off the rest of the saline particles from A, one after another, till it has got them all, or almost all, about itself. And when the metallic particles are thus divested of the nitrous ones, which, as a mediator between them and the water, held them floating in it; the alcalizate ones crouding for the room, the metallic ones took up before, will press these towards one another, and make them come

more easily together: so that by the motion they continually have in the water, they shall be made to strike on one another; and then, by means of the principle in the second supposition, they will cohere, and grow into clusters, and fall down by their weight to the bottom, which is called precipitation.

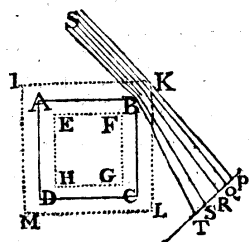
In the solution of metals, when a particle is loosing from the body, so soon as it gets to that distance from it, where the principle of receding, described in the 4th and 5th suppositions, begins to overcome the principle of acceding, described in the second supposition; the receding of the particle will be thereby accelerated, so that the particle shall, as it were with violence, leap from the body, and, putting the liquor into a brisk agitation, beget and promote that heat, we often find to be caused in solutions of metal. And if any particle happen to leap off thus from the body, before it be surrounded with water, or to leap off with that smartness as to get loose from the water, the water, by the principle, in the 4th and 5th suppositions, will be kept off from the particle, and stand round about it like a spherical hollow arch, not being able to come to a full contact with it any more. And several of these particles afterwards gathering into a cluster, so as by the same principle to stand at a distance from one another without any water between them, will compose a bubble. Whence I suppose it is, that, in brisk solutions, there usually happens an ebullition.

This is one way of transmuting gross compact substances into aerial ones. Another way is by heat. For as fast as the motion of heat can shake off the particles of water from the surface of

of

of it; those particles, by the said principle, will float up and down in the air at a distance both from one another, and from the particles of air, and make that substance we call vapour. Thus I suppose it is, when the particles of a body are very small, (as I suppose those of water are) so that the action of heat alone may be sufficient to shake them asunder. But if the particles be much larger, they then require the greater force of dissolving menstrooms to separate them, unless by any means the particles can be first broken into smaller ones. For the most fixed bodies, even gold itself, some have said, will become volatile only by breaking their parts smaller. Thus may the volatility, and fixedness of bodies, depend on the different sizes of their parts.

And on the same difference of size may depend the more or less permanency of aerial substances in their state of rarification. To understand this, let us suppose ABCD to be a large piece of any metal; EFGH, the limit of the interior uniform æther; and B, a part of the metal at the superficies AB. If this part, or



particle B, be so little, that it reaches not to the limit EF; it is plain that the æther, at its center, must be less rare than if the particle were greater: for were it greater, its center would be further from the superficies AB; that is, in a place where the æther, by supposition, is rarer. The less the particle B therefore, the denser the æther at its center; because its center comes nearer to the edge AB, where the æther is denser than within the limit EFGH. And if the particle were divided from the body, and removed to a distance from it, where the æther is still denser, the æther within it must proportionally grow denser. If you consider this, you may apprehend how, by diminishing the particle, the rarity of the æther within it will be diminished, till between the density of the æther without, and the density of the æther within it, there be little difference; that is, till the cause be almost taken away which should keep this, and other such particles, at a distance from one another. For that cause, explained in the 4th and 5th

suppositions, was the excess of density of the external æther above that of the internal. This may be the reason, then, why the small particles of vapours easily come together, and are reduced back into water; unless the heat, which keeps them in agitation, be so great as to dissipate them as fast as they come together: but the grosser particles of exhalations, raised by fermentation, keep their aerial form more obstinately, because the æther within them is rarer.

Nor does the size only, but the density of the particles also, conduce to the permanency of aerial substances. For the excess of density of the æther without such particles, above that of the æther within them, is still greater: which has made me sometimes think, that the true permanent air may be of a metallic original: the particles of no substances being more dense than those of metals. This I think is also favoured by experience; for I remember I once read in the Philosophical Transactions how Mr. *Hugens*, at *Paris*, found, that the air made by dissolving salt of tartar would, in two or three days time, condense and fall down again; but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then how, by the continual fermentations made in the bowels of the earth, there are aerial substances raised out of all kinds of bodies; all which together make the atmosphere, and that of all these, the metallic are the most permanent, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of these: especially since they are the heaviest of all other, and so must subside to the lower parts of the atmosphere, and float upon the surface of the earth, and buoy the lighter exhalation and vapours, to float in greatest plenty above them. Thus, I say, it ought to be with the metallic exhalations, raised in the bowels of the earth by the action of acid menstrooms; and thus it is with the true permanent air: for this, as in reason it ought to be esteemed the most ponderous part of the atmosphere, because the lowest; so it betrays its ponderosity, by making vapours ascend readily in it; by sustaining mists and clouds of snow; and by buoying up gross and ponderous smoak. The air also is the most gross, un-

active part of the atmosphere, affording living things no nourishment, if deprived of the more tender exhalations and spirits that float in it; and what more unactive, or remote from nourishment, than metallic bodies.

I shall set down one conjecture more, which came into my mind now as I was writing this letter: it is about the cause of gravity. For this end, I will suppose æther to consist of parts differing from one another in subtilty by indefinite degrees: that in the pores of bodies, there is less of the grosser æther in proportion to the finer, than in open spaces; and consequently, that in the great body of the earth there is much less of the grosser æther, in proportion to the finer, than in the regions of the air: and that yet the grosser æther in the air affects the upper regions of the earth, and the finer æther in the earth the lower regions of the air in such a manner, that, from the top of the air to the surface of the earth, and again from the surface of the earth to the center thereof, the æther is insensibly finer and finer. Imagine, now, any body suspended in the air, or lying on the earth; and the æther being, by the hypothesis, grosser in the pores which are in the upper parts of the body, than in those which are in the lower parts; and that grosser æther, being less apt to be lodged in those pores, than the finer æther below; it will endeavour to get out, and give way to the finer æther below, which cannot be, without the bodies descending to make room above for it to go out into.

From this supposed gradual subtilty of the parts of the æther, some things above might be further illustrated, and made more intelligible; but by what has been said, you will easily discern whether, in these conjectures, there be any degree of probability; which is all I aim at. For my own part, I have so little fancy to things of this nature, that, had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon in yours, &c.

D E

N A T U R A A C I D O R U M.

E c c 2

D E

N A T U R A A C I D O R U M.

ACIDORUM particulæ sunt aqueis crassiores, & propterea minus volatiles; at terrestribus multo subtiliores, & propterea multo minus fixæ. Vi magnâ attractivâ pollent, & in hac vi consistit earum activitas; quâ & corpora dissolvunt, & organa sensuum agitant & pungunt. Mediæ sunt naturæ inter aquam & corpora, & utraque attrahunt. Per vim suam attractivam congregantur circum particulas corporum, seu lapideas seu metallicas, iisque undique adhærent arctissimè, ut ab iisdem deinceps per distillationem vel sublimationem vix possint separari; attractæ verò & undique congregatæ elevant disjungunt ac discutiunt particulas corporum ab invicem; id est, corpora dissolvunt; & per vim attractionis, quâ ruunt in corpora, commovent fluidum, & calorem excitant; particulasque nonnullas adeo discutiunt, ut in aërem convertant, & bullulas generant. Et hæc est ratio dissolutionis & fermentationis violentæ. Acidum verò, attrahendo aquam æquæ ac particulas corporum, efficit ut particulæ dissolutæ promptè miscantur cum aquâ, eique innatent ad modum salium. Et quemadmodum globus terræ per vim gravitatis, attrahendo aquam fortius quàm corpora leviora, efficit ut leviora ascendant in aquâ & fugiant in terra; sic particulæ salium, attrahendo aquam, fugant se mutuo, & ab invicem quàm maximè recedendo, per aquam totam expanduntur.

Particulæ

Particulæ falis Alkali ex terreis et acidis similiter unitis constant; sed hæ acidæ vi magnâ attractivâ pollent, ut per ignem non separantur à sale, utque metalla dissoluta præcipitent, attrahendo ab ipsis particulas acidas quibus dissolvebantur.

Si particulæ acidæ in minori proportionem cum terrestribus jungantur, tam arctè retinentur à terrestribus, ut ab iis supprimi ac occultari videantur; neque enim sensum jam pungunt neque attrahunt aquam; sed corpora dulcia, & quæ cum aquâ ægrè miscentur, hoc est pingua componunt: ut fit in Mercurio dulci, sulphure communi, Lunâ Corneâ, & Cupro, quod Mercurius sublimatus corrodit. Ab acidi verò sic suppressi vi attractivâ fit, ut pingua corporibus propè universis adhæreant, & flammam facili concipiant; si modò acidum calefactum inveniat alias corporum in fumo accensorum particulas, quas fortius attrahat quàm proprias: sed & acidum in sulphureis suppressum, fortius attrahendo particulas aliorum corporum, scilicet terreas, quàm proprias, fermentationem brutam & naturalem ciet, & fovet, usque ad putrefactionem compositi; quæ putrefactio sita est in eo, quòd Acidi *particulæ* fermentationem diu foventes, tandem in interstitia minima et partes primæ compositionis interjacentia sese insinuant, intimèque iis partibus unitæ mixtionem novam efficiunt, non amovendam nec cum priore commutandam.

Cogitationes variæ.

Flamma est fumus candens, differtque à fumo, ut ferrum rubens ab ignito sed non rubente.

Calor est agitatio partium quaquaverfum.

Nihil est absolutè quiescens secundum partes suas, & ideo frigidum, præter atomos, vacui scilicet expertes.

Terra augetur aquâ in eam conversâ, et omnia in aquam reduci possunt.

Nitrum abit distillatione magnam partem in spiritum acidum, relicta terrâ; quia acidum nitri attrahit phlegma, et idcirco simul ascendunt, constituuntque spiritum. At nitrum carbone accensum magnam partem abit in Sal Tartari, quia ignis eo modo applicatus partes acidi & terræ in se impingit, fortiusque unit.

Spiritus ardentes sunt Olea cum phlegmate, per fermentationem, unita.

Tinctura Cochinellæ, cum spiritu Vini facta, in aquæ magnam molem immissa, parvâ licet dosi, totam tamen aquam inficit; scilicet quia particulæ Cochinellæ magis attrahuntur ab aquâ, quàm à se mutuo.

Aqua non habet magnam vim dissolvendi, quia paucò acido gaudet; acidum enim dicimus, quod multum attrahit & attrahitur. Videmus nempe ea quæ in aqua solvuntur, lentè & sine effervescentiâ solvi; at ubi est attractio fortis, & particulæ menstrui undique attrahuntur à particulâ metalli; vel potius particula metalli undique attrahitur à particulis menstrui, hæ illam abripiunt, & circumfistunt; hoc est, metallum corrodunt; hæ eadem particulæ sensorio applicatæ ejus partes eodem modo divellunt, doloremque inferunt. Acidæ appellantur relicta scilicet terrâ subtili cui adhærebant, ob majorem attractionem ad liquidum linguæ, &c.

In omni solutione per menstruum, particulæ solvendæ magis attrahuntur à partibus menstrui quàm à se mutuo.

In omni fermentatione est acidum suppressum, quod coagulat præcipitando.

Oleum, cum nimis magnâ mole phlegmatis intimè mixtum, fit salinum quiddam, & sic acetum constituit; hinc etiam Tartari, seu terræ admistæ, habenda est ratio.

Mercurius attrahitur, i. e. corroditur ab acidis; & sicut pondere obstructions tollit, ita vi attractrice acida infringit.

Mercurius est volatilis, & faciliè elevatur calore; quia ejus particulæ ultimæ compositionis sunt parvæ, & faciliè separantur, separatæque sese fugant; ut fit in particulis vaporis fluidorumque rarefactorum.

Aqua comprimi non potest; quia ejus particulæ jamjam se tangunt: ut si se tangerent particulæ aëris (comprimit aer potest quia ipse particulæ nondum se tangunt) aer evaderet in marmor. Sequitur ex Prop. xxiii. Lib. 2. Princip. Philos.

Aurum particulas habet se mutuòtrahentes; harum summæ vocantur primæ compositionis; harum summarum summæ, secundæ, &c.

D E N A T U R A, &c.

Potest Mercurius, potest Aqua Regis poros pervadere, qui particulas ultimæ compositionis interjacent, at non alios. Si possent menstruum alios illos pervadere, vel si auri partes primæ & secundæ compositionis possent separari, fieret aurum fluidum & malleabile. Si aurum fermentescere possent, in aliud quodvis corpus possent transformari.

Visciditas est, vel solum defectus fluiditatis (quæ sita est in partium parvitate & sic separabilitate, intellige partes ultimæ compositionis) vel defectus lubricitatis, seu lævoris, partes unas supra alias labi impediens. Hujus visciditatis acidum sæpe causa est, sæpe spiritus alius lubricus terræ junctus; ut Oleum Terebinthinæ, capiti suo mortuo redditum, fit tenax.

Ratio cur charta oleo inuncta transitum oleo, non Aquæ, concedat, est, quia aqua Oleo non miscetur, sed fugatur ab eo.

Acidum primigenium videtur constare particulis sphaericis, hoc est summè attractivis; & particulæ hæ sunt majores aquæ, & minores terræ partibus, seu inter eas mediæ & utrarumque attractrices.

Cum acidæ partes, minores scilicet, aliquod dissolvunt, id faciunt, quia partem rei solvendæ includunt undique, utpote majorem quolibet acidi partium.

T A B U L Æ D U Æ

C A L O R U M A L T E R A

A L T E R A R E F R A C T I O N U M.

I.

TABULA QUANTITATUM

ET

GRADUUM CALORIS.

Colorum partes æquales.	Colorum gradus.	Calorum Descriptiones et Signa.
		C ALOR aëris hyberni ubi aqua incipit gelu rigescere. Innotescit hic calor accuratè locando Thermometrum in nive compressâ, quo tempore gelu solvitur.
0, 1, 2.		Calores aëris hyberni.
2, 3, 4.		Calores aëris verni & autumnalis.
4, 5, 6.		Calores aëris æstivi.
6.		Calor usitatus aëris meridiani circa mensẽ Julium.
12.	I	Calor maximus quem Thermometer ad contactum corporis humani concipit. Idem circiter est calor avis ova incubantis.
14 $\frac{3}{11}$	I $\frac{1}{4}$	Calor balnei propè maximus quem quis, manu immersâ & constanter agitatâ, diutiùs perferre potest. Idem ferè est calor sanguinis recens effusi.
17	I $\frac{1}{4}$	Calor balnei maximus quem quis, manu immersâ & immobili manente, diutiùs perferre potest.
20 $\frac{1}{11}$	I $\frac{3}{4}$	Calor balnei quo cera innatans & liquefacta defervescendo rigescit, & diaphaneitatem amittit.
		F f f 2 Calor

24	2	Calor balnei quo cera innatans incallescendo liquefit, & in continuo fluxu sine ebullitione conservatur.
28 $\frac{6}{11}$	2 $\frac{1}{4}$	Calor mediocris inter calores quo cera liquefit, & aqua ebullit.
34	2 $\frac{1}{2}$	Calor quo aqua vehementer ebullit; & mistura duarum partium plumbi, trium partium stanni & quinque partium bismuti defervescendo rigescit. Incipit aqua ebullire calore partium 33, & calorem partium plusquam 34 $\frac{1}{2}$ ebulliendo vix concipit. Ferrum verò defervescens calore partium 35 vel 36, ubi aqua calida, & 37, ubi frigida in ipsum guttatim incidit, definit ebullitionem excitare.
40 $\frac{4}{11}$	2 $\frac{3}{4}$	Calor minimus quo mistura unius partis plumbi, quatuor partium stanni, & quinque partium bismuti incallescendo liquefit, & in continuo fluxu conservatur.
48	3	Calor minimus quo mistura æqualium partium stanni & bismuti liquefit. Hæc mistura calore partium 47 defervescendo coagulatur.
57	3 $\frac{1}{4}$	Calor quo mistura duarum partium stanni, & unius partis bismuti funditur, ut & mistura trium partium stanni & duarum plumbi: sed mistura quinque partium stanni & duarum partium bismuti hoc calore defervescendo rigescit. Et idem facit mistura æqualium partium plumbi & bismuti.
68	3 $\frac{1}{2}$	Calor minimus quo mistura unius partis bismuti, & octo partium stanni funditur. Stannum per se funditur calore partium 72, & defervescendo rigescit calore partium 70.
81	3 $\frac{3}{4}$	Calor quo bismutum funditur; ut & mistura quatuor partium plumbi, & unius partis stanni. Sed mistura quinque partium plumbi, & unius partis stanni, ubi fusa est & defervescit, in hoc calore rigescit.
96	4	Calor minimus quo plumbum funditur. Plumbum incallescendo funditur calore partium 96 vel 97, & defervescendo rigescit calore partium 95.

Calor

114	4 $\frac{1}{4}$	Calor quo corpora ignita, defervescendo, penitus desinunt in tenebris nocturnis lucere; & vicissim, incallescendo, incipiunt in iisdem tenebris lucere, sed luce tenuissimâ quæ sentiri vix possit. Hoc calore liquefit mistura æqualium partium stanni & reguli martis; & mistura septem partium bismuti & quatuor partium ejusdem Reguli defervescendo rigescit.
136	4 $\frac{1}{2}$	Calor quo corpora ignita in tenebris nocturnis candent, in crepusculo verò neutiquam. Hoc calore tum mistura duarum partium reguli martis & unius partis bismuti, tum etiam mistura quinque partium reguli martis & unius partis stanni defervescendo rigescit. Regulus per se rigescit calore partium 146.
161	4 $\frac{3}{4}$	Calor quo corpora ignita in crepusculo, proximè ante ortum solis, vel post occasum ejus, manifestò candent; in clarâ verò diei luce neutiquam, aut non nisi perobscurè.
192	5	Calor prunarum in igne parvo culinari, ex carbonibus fossilibus bituminosis constructo, & absque usu follium ardente. Idem est calor ferri, in tali igne quantum potest candentis. Ignis parvi culinarius, qui ex lignis constat, calor paulo major est; nempe partium 200 vel 210. Et ignis magni major adhuc est calor; præsertim si folliis cieatur.

In hujus Tabulæ columnâ primâ habentur gradus caloris in proportionem arithmeticâ; computum inchoando à calore, quo aqua incipit gelu rigescere, tanquam ab infimo caloris gradu, seu communi termino caloris & frigoris; & ponendo calorem externum corporis humani esse partium duodecim. In secundâ columnâ habentur gradus caloris in ratione geometricâ; sic ut secundus gradus sit duplo major primo, tertius item secundo, & quartus tertio, & primus sit calor externus corporis humani sensibus æquantus. Patet autem per hanc Tabulam, quòd calor aquæ bullientis sit ferè triplo major quàm calor corporis humani; & quòd calor stanni liquefcentis sit sextuplo major, & calor plumbi liquefcentis octuplo major, & calor Reguli liquefcentis duodecuplo major,

jor, & calor ordinarius ignis culinaris sexdecim vel septemdecim vicibus major, quàm calor idem corporis humani.

Constructa fuit hæc Tabula ope Thermometri & Ferri candentis. Per Thermometrum inveni mensuram calorum omnium usque ad calorem quo stannum funditur, & per ferrum calefactum inveni mensuram reliquorum. Nam calor quem ferrum calefactum corporibus frigidis sibi contiguis dato tempore communicat, hoc est, calor quem ferrum dato tempore amittit, est ut calor totus ferri. Ideoque si tempora refrigerii sumantur æqualia, calores erunt in ratione geometricâ, & propterea per tabulam logarithmorum facillè inveniri possunt.

Primum igitur per Thermometrum, ex oleo lini constructum, inveni, quòd si oleum, ubi Thermometer in nive liquefcente locabatur, occupabat spatium partium 10000; idem oleum, calore primi gradûs seu corporis humani rarefactum, occupabat spatium 10256; & calore aquæ jamjam ebullire incipientis, spatium 10705; & calore aquæ vehementer ebullientis, spatium 10725; & calore stanni liquefacti defervescentis, ubi incipit rigescere, & consistentiam amalgamatis induere, spatium 11516; & ubi omnino rigescit, spatium 11496. Igitur oleum rarefactum fuit ad dilatatum in ratione 40 ad 39, per calorem corporis humani; in ratione 15 ad 14, per calorem aquæ bullientis; in ratione 15 ad 13, per calorem stanni defervescentis, ubi incipit coagulari & rigescere; & in ratione 23 ad 20, per calorem quo stannum defervescens omnino rigescit. Rarefactio aëris æquali calore fuit decuplo major quàm rarefactio olei; & rarefactio olei quasi quindecim vicibus major quàm rarefactio spiritûs vini. Et ex his inventis, ponendo calores olei ipsius rarefactioni proportionales, & pro calore corporis humani scribendo partes 12, prodiit calor aquæ ubi incipit ebullire, partium 33; & ubi vehementius ebullit, partium 34; & calor stanni, ubi vel liquefcit, vel defervescendo incipit rigescere, & consistentiam amalgamatis induere, prodiit partium 72; & ubi defervescendo rigescit, & induratur, partium 70.

His cognitis, ut reliqua investigarem, calefeci ferrum satis crassum, donec satis canderet; & ex igne cum forcipe etiam candente exemptum locavi statim in loco frigido, ubi ventus constanter spirabat; & huic imponendo particulas diversorum metallorum, &

aliorum

aliorum corporum liquabilium, notavi tempora refrigerii; donec particulæ omnes, amissâ fluiditate, rigescerent, & calor ferri æquaretur calori corporis humani. Deinde ponendo quòd excessus calorum ferri & particularum rigescentium supra calorem atmosphææ, Thermometro inventum, essent in progressionem geometricâ, ubi tempora sunt in progressionem arithmeticâ, calores omnes innotuere. Locavi autem ferrum, non in aëre tranquillo, sed in vento uniformiter spirante; ut aër à ferro calefactus semper abriperetur à vento, & aër frigidus in locum ejus uniformi cum motu succederet. Sic enim aëris partes æquales æqualibus temporibus calefactæ sunt, & calorem conceperunt calori ferri proportionalem.

Calores autem sic inventi eandem habuerunt rationem inter se cum caloribus per Thermometrum inventis; & propterea rarefactiones olei ipsius caloribus proportionales esse rectè assumpsimus.

II.

Tabula Refractionum Siderum ad Altitudines apparentes.

Alt. Appar. deg. m.	Refractio. m. sec.	Alt. Appar. deg.	Refractio. m. sec.	Alt. Appar. deg.	Refractio. m. sec.
0 0	33 45	16	3 4	46	0 52
0 15	30 24	17	2 53	47	0 50
0 30	27 35	18	2 43	48	0 48
0 45	25 11	19	2 34	49	0 47
1 0	23 7	20	2 26	50	0 45
1 15	21 20	21	2 18	51	0 44
1 30	19 46	22	2 11	52	0 42
1 45	18 22	23	2 5	53	0 40
2 0	17 8	24	1 59	54	0 39
2 30	15 2	25	1 54	55	0 38
3 0	13 20	26	1 49	56	0 36
3 30	11 57	27	1 44	57	0 35
4 0	10 48	28	1 40	58	0 34
4 30	9 50	29	1 36	59	0 32
5 0	9 2	30	1 32	60	0 31
5 30	8 21	31	1 28	61	0 30
6 0	7 45	32	1 25	62	0 28
6 30	7 14	33	1 22	63	0 27
7 0	6 47	34	1 19	64	0 26
7 30	6 22	35	1 16	65	0 25
8 0	6 0	36	1 13	66	0 24
8 30	5 40	37	1 11	67	0 23
9 0	5 22	38	1 8	68	0 22
9 30	5 6	39	1 6	69	0 21
10 0	4 52	40	1 4	70	0 20
11 0	4 27	41	1 2	71	0 19
12 0	4 5	42	1 0	72	0 18
13 0	3 47	43	0 58	73	0 17
14 0	3 31	44	0 56	74	0 16
15 0	3 17	45	0 54	75	0 15

D E

PROBLEMATIS BERNOULLIANIS.

PRÆNOBILI VIRO
D. CAROLO MONTAGUE

Armig. Scæcarti Cancellari, & Societatis Regiæ Præsidi.

Jan. 30, 1695.

Accepi, Vir Amplissime, hesterno die duo Problematum, à
JOHANNE BERNOULLIO Mathematicorum acutissimo propositorum,
exemplaria, Groningæ edita in hæc verba

Acutissimis qui toto Orbe florent Mathematicis S. P. D.

JOHANNES BERNOULLI, MATH. P. P.

CUM compertum babeamus, vix quicquam esse, quod magis ex-
citet generosa ingenia ad moliendum quod conducit augendis
scientiis, quàm difficilium pariter & utilium questionum propositio-
nem; quarum enodatione, tanquam singulari, siquid aliud videtur, ad
nominis claritatem perveniant sibi quæ apud posteritatem æterna
extruant monumenta: sic me nihil gratius orbi mathematico factu-
rum

rum speravi, quàm si, imitando exemplum tantorum virorum, MER-
SENNI, PASCALII, FERMATII, præsertim recentis illius anonymi
æigmatistæ Florentini *, aliorumque qui idem ante me fecerunt,
præstantissimis bujus ævi analysi proponerem aliquod Problema, quo,
quasi lapide Lydio, suas methodos examinare, vires intendere, &
si quid invenirent, nobiscum communicare possent; ut quisque suas
exinde promeritas laudes à nobis, publicè id profitentibus, conseque-
retur.

Factum autem illud est ante semestrem in Actis Lipf. m. Jun. pag.
269, ubi tale problema propositi, cuius solutio cum jucunditate
conjunctam videbunt omnes, qui cum successu ei se applicabunt.
Sex mensium spatium à primâ publicationis die Geometris concessum
est, intra quod, si nulla solutio prodiret in lucem, me meam exhi-
biturum promissi. Sed ecce elapsus est terminus, & nihil solutionis
comparuit; nisi quod Celeb. LEIBNITIUS, de profundiore geometriâ
præclare meritis, me per litteras certiore fecerit, se jam feliciter
dissolvissè nodum pulcherrimi bujus, uti vocabat, & inauditi antea
problematis; insimulque humaniter rogavit, ut præstitutum limitem
ad proximum pascha extendi paterer; quo interea apud Gallos, Ita-
lòsque, idem illud publicari posset, nullusque adeò superesset locus ulli
de angustia termini querele. Quam honestam petitionem non so-
lum indulsi, sed ipse hanc prorogationem promulgare decrevi; visu-
rus num qui sint, qui nobilem hanc & arduam quæstionem aggres-
suri, post longum temporis intervallum, tandem enodationis compotes
fierent. Illorum interim in gratiam, ad quorum manus Acta Lip-
sienfia non perveniunt, propositionem hîc repeto.

* Vincentius VIVIANI ann. 1691. ænigma geometricum proposuit, de miro opificio Testudinis
quadrabilis hemisphericæ; videantur Acta Eruditorum hujus anni, mense Junio, pag. 274, vel
Vita VIVIANI in Hist. Acad. Reg. Scient. Paris. ann. 1703.

P R O B.

P R O B. I.

Determinare lineam curvam data duo puncta, in diversis ab ho-
rizonte distantis & non in eadem rectâ verticali posita, connec-
tentem, super quâ mobile, propriâ gravitate decurrens & à
superiori puncto moveri incipiens, citissimè descendat ad punc-
tum inferius.

Sensus problematis hic est: ex infinitis lineis, quæ duo illa data
puncta conjungunt, vel ab uno ad alteram duci possunt, eligatur illa,
juxta quam si incurvetur lamina tubi canalifve formam habens,
ut ipsi impositus globulus & liberè dimissus, iter suum ab uno puncto
ad alterum emetiat tempore brevissimo.

Ut verò omnem ambiguitatis ansam præcaveamus; scire B. L.
volumus, nos hîc admittere GALILÆI hypotbesin, de cujus veri-
tate, sepositâ resistantiâ, jam nemo est saniorum geometrarum qui
ambigat; velocitates scilicet acquisitas gravium cadentium esse
in subduplicatâ ratione altitudine emensarum: quamquam aliâs
nostra solvendi methodus universaliter ad quamvis aliam hypotbesin
sefe extendat.

Cum itaque nihil obscuritatis supersit, obnixè rogamus omnes &
singulos bujus ævi geometras, accingant se promptè, tentent, discus-
sant quicquid in extremo suarum methodorum recessu absconditum
tenent. Rapiat qui potest præmium, quod solutori paravimus; non
quidem auri, non argenti summam, quo abjecta tantum & merce-
naria conducuntur ingenia, à quibus ut nihil laudabile, sic nihil,
quod scientiis fructuosum, expectamus; sed cum virtus sibi ipsi sit
merces pulcherrima, atque gloria immensum habeat calcar, offeri-
mus præmium, quale convenit ingenui sanguinis viro, consertum ex
honore, laude, & plausu; quibus magni nostri Apollinis perspicaci-
tatem, publicè & privatim, scriptis & dictis coronabimus, conde-
corabimus, & celebrabimus.

Quòd si verò festum paschatis præterierit, nemine deprehenso, qui
quæsitum nostrum solverit; nos quæ ipsi invenimus publico non invi-
debimus: incomparabilis enim LEIBNITIUS solutiones tum suam,
tum nostram, ipsi jam pridem commissam, protinus, ut spero, in lu-
cem emittet; quas si geometræ, ex penitiori quodam fonte petitas,
perspexerint,

perspexerint, nulli dubitamus, quin angustos vulgaris geometriæ limites agnoscant, nostraque proin inventa tanto pluris faciant, quanto pauciores eximiam nostram quæstionem soluturi extiterint, etiam inter illos ipsos, qui per singulares, quas tantoperè commendant, methodos, interioris geometriæ latibula non solum intimè penetrâsse, sed etiam ejus pomæria, theorematibus suis aureis, nemini, ut putabant, cognitis, ab aliis tamen jam longè priùs editis, mirum in modum extendisse gloriantur.

PROBLEMA ALTERUM
PURE GEOMETRICUM.

Quod priori subnectimus, & Strenæ loco Eruditis proponimus.

Ab EUCLIDIS tempore vel tyronibus notum est, ductam utcumque à puncto dato rectam lineam, à circuli peripheriâ ita secari, ut rectangulum duorum segmentorum, inter punctum datum & utramque peripheriæ partem interceptorum, sit eidem constanti perpetuò æquale. Primus ego ostendi, in eodem Actorum Jun. pag. 265, hanc proprietatem infinitis aliis curvis convenire, illamque adèò circulo non esse essentialem: arreptâ hinc occasione, proposui geometris determinandam Curvam, vel Curvas, in quibus non rectangulum, sed solidum sub uno & quadrato alterius segmentorum æquetur semper eidem: sed à nemine hactenus solvendi modus prodiit; exhibebimus eum, quandocumque desiderabitur. Quoniam autem non nisi per Curvas transcendentes quæsito satisfacimus; en aliud, cujus solutio per merè algebraicas in nostrâ est potestate.

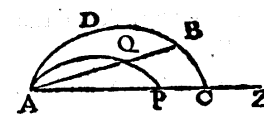
Quæritur Curva ejus proprietatis, ut duo illa segmenta, ad quamcunque potentiam datam elevata & simul sumta, faciant ubique unam eandemque summam.

Casum simplicissimum, existente scilicet numero potentiæ 1, ibidem in Actis, pag. 266, jam solutum dedimus; generalem verò solutionem, quam etiamnum premimus, analysis eruendam relinquimus.

Dabam Groningæ, ipsis Cal. Jan. 1697.

HACTENUS BERNOULLIUS. Problematum verò solutiones sunt bujusmodi.

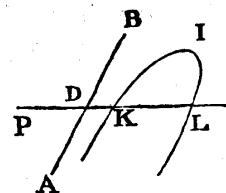
A dato



A dato puncto A, ducatur recta infinita, APCZ, horizonti parallela; & super eadem rectâ describatur, tum Cyclois quæcunque, AQP, rectæ AB ductæ, & si opus sit productæ, occurrens in puncto Q, tum cyclois alia ABC; cujus basis, & altitudo sit ad prioris basem & altitudinem respectivè, ut AB ad AQ. Et hæc cyclois novissima transibit per punctum B, & erit curva illa linea, in quâ grave à puncto A ad punctum B vi gravitatis suæ citissimè perveniet. Q. E. I.

PROBLEMA II.

Problema alterum, si rectè intellexi (nam, quæ in *Actis Lips.* ab auctore citantur ad id spectantia nondum vidi) sic proponi potest. Quæritur Curva KIL eâ lege, ut si recta PKL à dato quodam puncto P, ceu polo, utcumque ducatur, & eidem curvæ in punctis duobus K, & L occurrat; potestates duorum ejus segmentorum, PK, & PL, à dato illo puncto P ad occursum illos ductorum, si sint æquæ altæ, (id est, vel quadrata, vel cubi, vel quadrato-quadrata, &c.) datam summam $PKq + PLq$ vel $PK\text{ cub.} + PL\text{ cub.}$ &c. (in omni rectæ illius positione) conficiant.



SOLUTIO.

Per datum quodvis punctum, A, ducatur recta quævis infinita positione data, ADB, rectæ mobili PKL occurrens in D: & nominentur AD, x; & PK, vel PL, y; sintque Q, & R quantitates ex quantitibus quibuscunque datis, & quantitate x quomodocunque constantes, & relatio inter x, & y, definiatur per hanc æquationem $yy + Qy + R = 0$. Et si R sit quantitas data, rectangulum sub segmentis PK & PL dabitur. Si Q sit quantitas data, summa segmentorum illorum, sub signis propriis conjunctorum, dabitur^(a). Si $QQ - 2R$ datur, summa quadratorum ($PKq + PLq$) dabitur. Si $Q^3 - 3QR$ data sit quantitas, summa cuborum ($PK\text{ cub.} + PL\text{ cub.}$) dabitur. Si $Q^4 - 4QQR + 2RR$ data sit quantitas, summa quadrato-

^(a) Sequuntur hæc ex notissimo Theoremate Girardi de coefficientium procreatione. Vide Arith. Univ. C. XVIII.

quadratorum

quadratorum ($PKqq + PLqq$) dabitur. Et sic deinceps in infinitum (*). Efficiatur itaque ut R , Q , $QQ - 2R$, $Q^3 - 3QR$, &c. datae sint quantitates, & problema solvetur. Q. E. F.

Ad eundem modum Curvæ inveniri possunt, quæ tria, vel plura abscindunt segmenta, similes proprietates habentia. Sit æquatio $y^3 + Qyy + Ry + S = 0$, ubi Q , R , & S , quantitates significant ex quantitatibus quibuscunque datis, & quantitate x utcunque constantes, & Curva abscindet segmenta tria. Et si S data sit quantitas contentum solidorum illorum trium dabitur. Si Q sit quantitas data summa trium illorum dabitur. Si $QQ - 2R$ sit data quantitas, summa quadratorum ex tribus illis dabitur.

IV.

De ratione temporis, quo grave labitur per rectam data duo puncta conjungentem ad tempus brevissimum, quo, vi gravitatis, transit ab eorum uno ad alterum per arcum Cycloidis.

T H E O R E M A.

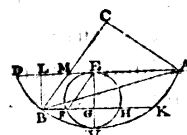
Si in Cycloide AVD , cujus basis AD est horizonti parallela, vertice V deorsum spectante, ex A ducatur utcunque recta AB Cycloidi occurrens in B , ex quo ducatur recta BC curvæ Cycloidis BD in B normalis, ad quam ex A demittatur perpendicularis recta AC : dico tempus, quo grave è quiete cadens ex A , vi suæ gravitatis, decurrit rectam AB , esse ad tempus, quo percurrit curvam AVB , sicut recta AB ad rectam AC .

Per B ducatur BL , parallela Cycloidis axi VE ; & BK , basi AD parallela occurrens axi in G , & circulo super diametrum EV descripto, in F & H ; Cycloidi denique in K . Ducatur recta EF , quæ, ex Cycloidis naturâ, parallela est rectæ BC . Unde BM est æqualis EF , & EM æqualis BF ; quæ, propter Cycloidem, æquatur arcui VF ; & proinde AM est æqualis arcui $EHVF$.

Per Prop. xxv. Par. II. *Horologii Oscillatorii* HUGENII, tempus, quo grave è quiete cadens percurrit AV , est ad tempus casus per EV , ut semi-circumferentia ad diametrum; & per dictæ partis propositionem ultimam, tempus, quo grave percurrit VB , post decursam AV (nempe æquale tempori, quo grave percurrit KV ,

(*) Patent hæc per formulas Girardi pro consummatione potestatum è radicibus æquationum cognititis. Vide Arithmet. Univ. C. XVIII. Not. 1.

post



post decursam AK), est ad tempus lapsus per AV , sicut arcus VF ad semi-circumferentiam; adeoque ad tempus casus per EV , sicut EV ad diametrum, quare tempus, quo grave percurrit curvam AVB , est ad tempus casus per EV , sicut arcus $EHVF$, ad diametrum EV . Sed, tempus casus per EV , est ad tempus casus per LB (five EG), sicut EV ad EF . Igitur, ex æquo, tempus, quo grave percurrit AVB , est ad tempus casus per LB , sicut arcus $EHVF$ ad subtensam EF ; hoc est ut recta AM , ad rectam MB . Rursus, tempus casus per LB est ad tempus lapsus per AB , ut LB ad AB . Ergo ratio temporis, quo grave percurrit AVB , ad tempus quo percurrit AB , componitur ex ratione AM ad MB , & ratione LB ad BA ; adeoque æqualis est rationi $AM \times LB$ ad $MB \times BA$. Sed $AM \times LB$ est æquale $MB \times AC$; quia utrumque æquatur duplo trianguli ABM . Et igitur, tempus, quo grave è quiete cadens percurrit curvam Cycloidis AVB , est ad tempus, quo percurrit rectam AB , sicut $MB \times AC$, ad $MB \times BA$; id est, sicut AC ad AB . Q. E. D.

Similiterque procedet demonstratio, si punctum B , sit inter A & V .

V.

P R O B L E M A T I S

Olim in Actis Eruditorum Lipsiæ propositi

S O L U T I O G E N E R A L I S.

In *Actis Eruditorum* pro mense Octobri anni 1698, pag. 471, D. JOHANNES BERNOULLIUS hæc scripsit. "Methodum, quam optaveram generalem secandi Curvas ordinatim positione datas, five algebraicas, five transcendentes, in angulo recto, five obliquo invariabili, five datâ lege variabili, tandem ex voto erui: cui, LEIBNITIO approbatore, ne $\gamma\gamma\delta$ addi potest ad ulteriorem perfectionem, & vel ideo tantum quod perpetuo ad æquationem deducat: in quâ si interdum indeterminatæ sunt inseparabiles, methodus non ideo imperfectior est, non enim hujus sed alius est methodi indeterminatas separare: rogamus

VOL. IV. H h h " igitur

“ igitur fratrem ut velit suas quoque vires exercere in re tanti
 “ momenti. Suscepti laboris non poenitebit, si felix successus
 “ fructu jucundo compensaverit. Scio relicturum suum, quem
 “ nunc fovet, modum, qui in paucissimis tantum exemplis adhi-
 “ beri potest.”

Illi tres viri celeberrimi sese, jam ab annis quatuor vel quinque
 circiter, in solvendis hujusmodi problematibus exercuerant. Abs-
 que spiritu divinandi eandem solutionem cum *Bernoullionâ* tradere
 difficile fuerit. Sufficit quod solutio sequens sit generalis, & ad
 æquationem semper deducatur.

P R O B L E M A.

*Quæritur methodus generalis inveniendi seriem Curvarum, quæ Cur-
 vas in serie aliâ quâcumque datâ constitutas, ad angulum vel
 datum, vel datâ lege variabilem, secabunt.*

S O L U T I O.

Natura Curvarum secandarum dat tangentes earundem ad in-
 tersectionum puncta quæcunque: & anguli intersectionum dant
 perpendiculara Curvarum secantium; & perpendiculara duo coeuntia,
 per concursum suum ultimum, dant centrum curvaminis Curvæ
 secantis ad punctum intersectionis cujuscunque. Ducatur abscissa
 in situ quocumque commodo, & sit ejus fluxio unitas; & positio
 perpendiculari dabit fluxionem primam ordinatæ ad Curvam quæsi-
 tam pertinentis; & curvamen hujus Curvæ dabit fluxionem se-
 cundam ejusdem ordinatæ. Et sic problema semper deducetur ad
 æquationes. Quod erat faciendum.

S C H O L I U M.

Non hujus, sed aliûs est methodi æquationes reducere, & in-
 determinatas separare; absolute, si fieri possit; sin minus, per se-
 ries infinitas. Problema hocce, cum nullius ferè sit usus, in
Actis Eruditorum annos plures neglectum & insolutum mansit.
 Et eadem de causâ solutionem ejus non ulterius prosequor.

P R O P O S I T I O N S

F O R

DETERMINING THE MOTION OF A BODY

U R G E D B Y

T W O C E N T R A L F O R C E S.

C O M M U N I C A T E D

B Y T H E L A T E W I L L I A M J O N E S, E S Q.

T O

T H E L A T E D R. B R A D L E Y.

[illegible]

SUPPOSE a body projected in the direction AP (fig. 1.) and acted upon by two centripetal forces towards the fixed points



T and s; and the angles PAS, PAT in different planes. Imagine the time divided into equal moments; and in the first moment the

the body, by its given force, should move through the line AP : likewise in the second moment, if no new force was added, it should continue to move in the same straight line through $PB = AP$. But when the body has come to P , suppose it acted upon by two centripetal forces, in the directions PT , PS . Suppose these forces in proportion to that in the direction AP , as the lines Pm , and Pn , to the line $AP = PB$; and with the three lines PB , Pn , Pm , complete the parallelepiped Pp ; and the body in P being acted on by these three forces, in the directions PB , Pm , Pn , which forces are as these three lines, it shall move through the diagonal of the parallelepiped made on these lines; that is, in the second moment of time the body, instead of moving from P to B , shall move from P to p . And drawing the lines sp , tp , and sa , ta , I say the solid $sptp$ is equal to the solid $satp$.

Draw the lines sb , tb . The solids $sptb$, $sptp$, having the same base spt , and being between the same parallel planes nm and bp , are equal. Again, the solids $sptb$, $spta$, are equal; because being on the same base spt , they have equal altitudes. (Letting fall perpendiculars from the points B and A , upon the same plane spt , in which from these perpendiculars, drawing lines to the points A and B , there are two right-angled triangles, with one side equal, *viz.* $PB = AP$; and one acute angle equal, *viz.* the inclination of the line AB to the plane spt , therefore the perpendiculars are equal, which are the altitudes of the two solids $sptb$, $spta$). Therefore the solids $sptp$, $satp$, are equal. Q. E. D.

In like manner, in the third moment of time, the body at p , being acted on by three forces in the direction pp , pt , ps , shall move through the line pq , so as to make the solids, $sptp$, $sptq$, equal. And so in every other moment of time, the body shall describe, by lines drawn from the two fixed points T and S , a little solid equal to the solid $satp$.

Thus then the moments of the solid described by the rays drawn from the fixed points, T , S , to the moving body, being proportional to the moments of time, it follows, that the solid itself is proportional to the time in which it is described. Q. E. D.

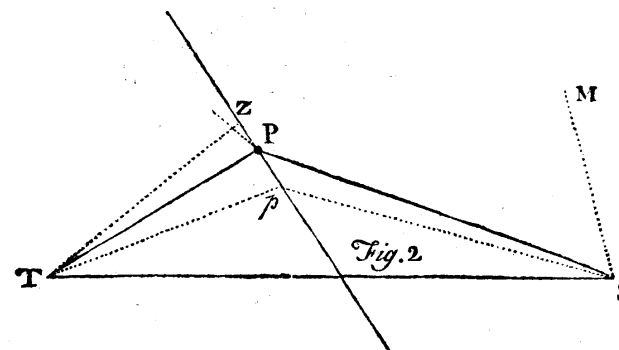
P R O P.

A DOUBLE CENTER.

P R O P. II.

If a body, acted upon by two centripetal forces, as above, revolve in a curve round the line joining these two centers, its velocity in any point of the curve shall be reciprocally proportional to a rectangle of the perpendicular from one of the centers upon the tangent to the given point in the curve, and the perpendicular from the other center upon the plane of the aforesaid perpendicular and tangent.

Let the body P move in the curve pp (fig. 2.) round the line ts , joining the two centers of attraction s and T . Let pz be a



tangent to the curve in P ; upon which, from T , let fall the perpendicular TZ ; and upon the plane of the lines TZ , zp let fall from S the perpendicular SM ; and the velocity of the body in P shall be reciprocally as the rectangle $TZ \times SM$.

Suppose in a moment of time the body moves from P to p , draw the lines ps and pt . The time being given, the velocity is directly as the space; that is, the velocity of the body at P is directly as the line pp .

Again, the time being given, the solid $tpps$, which is proportional to the time (by the preceding Proposition) is also given. But the solid $tpps$ is proportional to the base multiplied by the perpendicular altitude, that is, to the triangle tpp , into the perpendicular altitude sm . The triangle tpp is proportional to

 $pp \times$

FOUR
LETTERS
FROM
SIR ISAAC NEWTON
TO
DOCTOR BENTLEY;
CONTAINING
SOME ARGUMENTS
IN
PROOF OF A DEITY.

L E T T E R S, &c.

S I R,

WHEN I wrote my Treatise about our System, I had an eye upon such principles as might work with considering men, for the belief of a Deity; and nothing can rejoice me more than to find it useful for that purpose. But if I have done the public any service this way, it is due to nothing but industry and patient thought.

As to your first Query, it seems to me, that if the matter of our sun and planets, and all the matter of the universe, were evenly scattered throughout all the heavens, and every particle had an innate gravity towards all the rest, and the whole space, throughout which this matter was scattered, was but finite; the matter on the outside of this space would by its gravity tend towards all the matter on the inside, and by consequence fall down into the middle of the whole space, and there compose one great spherical mass. But if the matter was evenly disposed throughout an infinite space, it could never convene into one mass; but some of it would convene into one mass and some into another,
so

LETTER
FIRST.

so as to make an infinite number of great masses, scattered at great distances from one to another throughout all that infinite space. And thus might the sun and fixed stars be formed, supposing the matter were of a lucid nature. But how the matter should divide itself into two sorts; and that part of it, which is fit to compose a shining body, should fall down into one mass and make a sun; and the rest, which is fit to compose an opake body, should coalesce, not into one great body, like the shining matter, but into many little ones: Or if the sun at first were an opake body, like the planets, or the planets lucid bodies like the sun, how he alone should be changed into a shining body, whilst all they continue opake; or all they be changed into opake ones, whilst he remains unchanged; I do not think explicable by meer natural causes, but am forced to ascribe it to the counsel and contrivance of a voluntary Agent.

The same power, whether natural or supernatural, which placed the sun in the center of the fix primary planets, placed *Saturn* in the center of the orbs of his five secondary planets; and *Jupiter* in the center of his four secondary planets; and the earth in the center of the moon's orb; and therefore, had this cause been a blind one without contrivance or design, the sun would have been a body of the same kind with *Saturn*, *Jupiter*, and the earth; that is, without light or heat. Why there is one body in our system qualified to give light and heat to all the rest, I know no reason, but because the author of the system thought it convenient: and why there is but one body of this kind, I know no reason, but because one was sufficient to warm and enlighten all the rest. For the *Cartesian* hypothesis of suns losing their light, and then turning into comets, and comets into planets, can have no place in my System, and is plainly erroneous: because it is certain, that as often as they appear to us, they descend into the system of our planets, lower than the orb of *Jupiter*, and sometimes lower than the orbs of *Venus* and *Mercury*; and yet never stay here, but always return from the sun with the same degrees of motion by which they approached him.

cl 6

To

To your second Query I answer, that the motions, which the planets now have, could not spring from any natural cause alone, but were impressed by an intelligent Agent. For since comets descend into the region of our planets, and here move all manner of ways, going sometimes the same way with the planets, sometimes the contrary way, and sometimes in cross ways, the planes inclined to the plane of the ecliptick, and at all kinds of angles, it is plain that there is no natural cause which could determine all the planets, both primary and secondary, to move the same way and in the same plane, without any considerable variation: this must have been the effect of counsel. Nor is there any natural cause which could give the planets those just degrees of velocity, in proportion to their distances from the sun, and other central bodies, which were requisite to make them move in such concentrick orbs about those bodies. Had the planets been as swift as comets, in proportion to their distances from the sun (as they would have been, had their motion been caused by their gravity, whereby the matter, at the first formation of the planets, might fall from the remotest regions towards the sun) they would not move in concentrick orbs, but in such eccentrick ones as the comets move in. Were all the planets as swift as *Mercury*, or as slow as *Saturn* or his satellites; or were their several velocities otherwise much greater or less than they are, as they might have been, had they arose from any other cause than their gravities; or had the distances from the centers, about which they move, been greater or less than they are with the same velocities; or had the quantity of matter in the sun, or in *Saturn*, *Jupiter*, and the earth, and by consequence their gravitating power been greater or less than it is; the primary planets could not have revolved about the sun, nor the secondary ones about *Saturn*, *Jupiter*, and the Earth, in concentrick circles as they do, but would have moved in hyperbolas or parabolas, or in ellipses very eccentrick. To make this system, therefore, with all its motions, required a cause which understood, and compared together, the quantities of matter in the several bodies of the sun and planets, and the gravitating powers resulting from thence; the several distances of the primary planets from the sun, and of the secondary

condary ones from *Saturn*, *Jupiter*, and the Earth; and the velocities, with which these planets could revolve about those quantities of matter in the central bodies; and to compare and adjust all these things together in so great a variety of bodies, argues that cause to be not blind and fortuitous, but very well skilled in mechanicks and geometry.

To your third Query I answer, that it may be represented that the sun may, by heating those planets most which are nearest to him, cause them to be better concocted, and more condensed by that concoction. But when I consider that our earth is much more heated in its bowels below the upper crust, by subterraneous fermentations of mineral bodies than by the sun; I see not why the interior parts of *Jupiter* and *Saturn* might not be as much heated, concocted, and coagulated by those fermentations as our earth is: and therefore this various density should have some other cause than the various distances of the planets from the sun. And I am confirmed in this opinion by considering, that the planets of *Jupiter* and *Saturn*, as they are rarer than the rest, so they are vastly greater, and contain a far greater quantity of matter, and have many satellites about them; which qualifications surely arose not from their being placed at so great a distance from the sun, but were rather the cause why the Creator placed them at great distance. For by their gravitating powers they disturb one another's motions very sensibly, as I find by some late observations of Mr. *Flamsteed*; and had they been placed much nearer to the sun and to one another, they would by the same powers have caused a considerable disturbance in the whole system.

To your fourth Query I answer, that, in the hypothesis of *vortices*, the inclination of the axis of the earth might, in my opinion, be ascribed to the situation of the earth's *vortex* before it was absorbed by the neighbouring *vortices*, and the earth turned from a sun to a comet; but this inclination ought to decrease constantly in compliance with the motion of the earth's *vortex*, whose axis is much less inclined to the ecliptick; as appears by the motion of the moon carried about therein. If the sun by
his

his rays could carry about the planets, yet I do not see how he could thereby effect their diurnal motions. LETTER
FIRST.

Lastly, I see nothing extraordinary in the inclination of the earth's axis for proving a Deity; unless you will urge it as a contrivance for Winter and Summer, and for making the earth habitable towards the poles; and that the diurnal rotations of the sun and planets, as they could hardly arise from any cause purely mechanical, so by being determined all the same way with the annual and menstrual motions, they seem to make up that harmony in the system, which, as I explained above, was the effect of choice, rather than chance.

There is yet another argument for a Deity, which I take to be a very strong one; but till the principles on which it is grounded are better received, I think it more adviseable to let it sleep.

I am, &c;

Cambridge,
Dec. 10, 1692.

L E T T E R II.

S I R,

LETTER
SECOND.

I AGREE with you, that if matter, evenly diffused through a finite space, not spherical, should fall into a solid mass, this mass would affect the figure of the whole space, provided it were not soft, like the old chaos, but so hard and solid from the beginning, that the weight of its protuberant parts could not make it yield to their pressure. Yet by earthquakes loosening the parts of this solid, the protuberances might sometimes sink a little by their weight, and thereby the mass might, by degrees, approach a spherical figure.

The reason why matter evenly scattered through a finite space would convene in the midst, you conceive the same with me; but that there should be a central particle, so accurately placed in the middle, as to be always equally attracted on all sides, and thereby continue without motion, seems to me a supposition fully as hard, as to make the sharpest needle stand upright on its point upon a looking-glass. For if the very mathematical center of the central particle be not accurately in the very mathematical center of the attractive power of the whole mass, the particle will not be attracted equally on all sides. And much harder it is to suppose all the particles in an infinite space should be so accurately poised one among another, as to stand still in a perfect equilibrium. For I reckon this as hard as to make, not one needle only, but an infinite number of them (so many as there are particles in an infinite space) stand accurately poised upon their points. Yet I grant it possible, at least by a Divine Power; and if they were once to be placed, I agree with you, that they would continue in that posture without motion for ever, unless put into new motion by the same power. When therefore I said, that matter, evenly spread through all space, would convene by its gravity

gravity into one or more great masses, I understand it of matter LETTER
SECOND. not resisting in an accurate poise.

But you argue, in the next paragraph of your letter, that every particle of matter, in an infinite space, has an infinite quantity of matter on all sides, and by consequence an infinite attraction every way, and therefore must rest in equilibrio, because all infinities are equal. Yet you suspect a paralogism in this argument; and I conceive the paralogism lies in the position, that all infinities are equal. The generality of mankind consider infinities no other ways than indefinitely; and in this sense, they say all infinities are equal; though they would speak more truly if they should say, they are neither equal nor unequal, nor have any certain difference, or proportion one to another. In this sense, therefore, no conclusions can be drawn from them, about the equality, proportions, or differences of things; and they that attempt to do it, usually fall into paralogisms. So when men argue against the infinite divisibility of magnitude, by saying, that if an inch may be divided into an infinite number of parts, the sum of those parts will be an inch; and if a foot may be divided into an infinite number of parts, the sum of those parts must be a foot; and therefore since all infinities are equal, those sums must be equal, that is, an inch equal to a foot; the falliness of the conclusion shews an error in the premises: and the error lies in the position, that all infinities are equal. There is therefore another way of considering infinities, used by mathematicians, and that is under certain definite restrictions and limitations, whereby infinities are determined to have certain differences or proportions to one another. Thus Dr. Wallis considers them in his *Arithmetica Infinitorum*; where, by the various proportions of infinite sums, he gathers the various proportions of finite magnitudes: which way of arguing is generally allowed by mathematicians, and yet would not be good, were all infinities equal. According to the same way of considering infinities, a mathematician would tell you, that though there be an infinite number of infinite little parts in an inch, yet there is twelve times that number of such parts in a foot; that is, the infinite number of those parts in a foot is not equal to, but twelve

LETTER
SECOND.

times bigger than the infinite number of them in an inch. And so a mathematician will tell you, that if a body stood in equilibrio between any two equal and contrary attracting infinite forces, and if to either of these forces you add any new finite attracting force; that new force, how little soever, will destroy their equilibrium, and put the body into the same motion into which it would put it, were those two contrary equal forces but finite, or even none at all: so that in this case the two equal infinities, by the addition of a finite to either of them, become unequal in our ways of reckoning; and after these ways we must reckon, if from the considerations of infinities we would always draw true conclusions.

To the last part of your letter I answer, first, that if the earth (without the moon) were placed any where with its center in the *Orbis Magnus*, and stood still there without any gravitation or projection, and there at once were infused into it, both a gravitating energy towards the sun, and a transverse impulse of a just quantity moving it directly in a tangent to the *Orbis Magnus*; the compounds of this attraction and projection would, according to my notion, cause a circular revolution of the earth about the sun. But the transverse impulse must be a just quantity; for if it be too big or too little, it will cause the earth to move in some other line. Secondly, I do not know any power in Nature which would cause this transverse motion without the Divine arm. *Blondel* tells us somewhere in his book of Bombs, that *Plato* affirms, that the motion of the planets is such, as if they had all of them been created by God in some region very remote from our system, and let fall from thence towards the sun, and so soon as they arrived at their several orbs, their motion of falling turned aside into a transverse one. And this is true, supposing the gravitating power of the sun was double, at that moment of time in which they all arrive at their several orbs; but then the Divine power is here required in a double respect, namely, to turn the descending motions of the falling planets into a side motion, and at the same time to double the attractive power of the sun. So then gravity may put the planets into motion, but without the Divine Power it could never put them into such a circulating motion,

tion, as they have about the sun; and therefore for this, as well as other reasons, I am compelled to ascribe the frame of this system to an intelligent Agent.

You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know, and therefore would take more time to consider of it.

I fear what I have said of infinities will seem obscure to you; but it is enough if you understand that infinities, when considered absolutely without any restriction or limitation, are neither equal nor unequal, nor have any certain proportion one to another; and therefore the principle that all infinities are equal, is a precarious one.

I am, Sir, &c.

Trinity College,
Jan. 17, 1692-3.

L E T T E R III.

S I R,

Cambridge,
Feb. 25, 1692-3.

BECAUSE you desire speed, I will answer your letter with what brevity I can. In the six positions you lay down in the beginning of your letter, I agree with you. Your assuming the *Orbis Magnus* 7000 diameters of the earth wide, implies the sun's horizontal parallax to be half a minute. *Flamsteed* and *Cassini* have of late observed it to be about 10", and thus the *Orbis Magnus* must be 21,000, or in a rounder number 20,000 diameters of the earth wide. Either computation I think will do well, and I think it not worth while to alter your numbers.

In the next part of your letter you lay down four other positions founded upon the six first. The first of these four seems very evident; supposing you take attraction so generally, as by it to understand any force, by which distant bodies endeavour to come together without mechanical impulse. The second seems not

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not so clear. For it may be said, that there might be other systems of worlds before the present ones, and others before those, and so on to all past eternity; and by consequence, that gravity may be co-eternal to matter, and have the same effect from all eternity as at present; unless you have somewhere proved, that old systems cannot gradually pass into new ones; or that this system had not its original from the exhaling matter of former decaying systems, but from a chaos of matter evenly dispersed throughout all space. For something of this kind, I think, you say was the subject of your sixth Sermon; and the growth of new systems out of old ones, without the mediation of a Divine Power, seems to me apparently absurd.

The last clause of the second position I like very well. It is inconceivable, that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must do, if gravitation, in the sense of *Epicurus*, be essential and inherent in it. And this is one reason, why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.

Your fourth assertion, that the world could not be formed by innate gravity alone, you confirm by three arguments. But in your first argument you seem to make a *Petitio Principii*; for whereas many ancient philosophers and others, as well Theists as Atheists, have all allowed, that there may be worlds and parcels of matter innumerable or infinite, you deny this, by representing it as absurd, as that there should be positively an infinite arithmetical sum or number, which is a contradiction *in terminis*; but you do not prove it as absurd. Neither do you prove, that what

what men mean by an infinite sum or number, is a contradiction LETTER
THIRD. in nature; for a contradiction *in terminis* implies no more than an impropriety of speech. Those things, which men understand by improper and contradictory phrases, may be sometimes really in Nature without any contradiction at all: a silver irkhorn, a paper lanthorn, an iron whetstone, are absurd phrases; yet the things signified thereby are really in Nature. If any man should say, that a number and a sum, to speak properly, is that which may be numbered and summed, but things infinite are numberless, or, as we usually speak, innumerable and sumless, or insumnable, and therefore ought not to be called a number or sum; he will speak properly enough, and your argument against him will, I fear, lose its force. And yet if any man shall take the words, number and sum, in a larger sense, so as to understand thereby things, which in the proper way of speaking are numberless and sumless (as you seem to do when you allow an infinite number of points in a line) I could readily allow him the use of the contradictory phrases of innumerable number, or sumless sum, without inferring from thence any absurdity in the thing he means by those phrases. However, if by this, or any other argument, you have proved the finiteness of the universe, it follows, that all matter would fall down from the outides, and convene in the middle. Yet the matter in falling might concrete into many round masses, like the bodies of the planets; and these, by attracting one another, might acquire an obliquity of descent, by means of which they might fall, not upon the great central body, but upon the side of it, and fetch a compass about, and then ascend again by the same steps and degrees of motion and velocity with which they descended before, much after the manner that the comets revolve about the sun; but a circular motion in concentrick orbs about the sun, they could never acquire by gravity alone.

And though all the matter were divided at first into several systems, and every system by a Divine Power constituted like ours; yet would the outside systems descend towards the middlemost; so that this frame of things could not always subsist without a

Divine

LETTER
THIRD.

Divine Power to conserve it, which is the second argument; and to your third I fully assent.

As for the passage of *Plato*, there is no common place from whence all the planets being let fall, and descending with uniform and equal gravities (as *Galileo* supposes) would at their arrival to their several orbs acquire their several velocities, with which they now revolve in them. If we suppose the gravity of all the planets towards the sun to be of such a quantity as it really is, and that the motions of the planets are turned upwards, every planet will ascend to twice its height from the sun. *Saturn* will ascend till he be twice as high from the sun as he is at present, and no higher; *Jupiter* will ascend as high again as at present, that is, a little above the orb of *Saturn*; *Mercury* will ascend to twice his present height, that is, to the orb of *Venus*; and so of the rest; and then by falling down again from the places to which they ascended, they will arrive again at their several orbs with the same velocities they had at first, and with which they now revolve.

But if so soon as their motions by which they revolve are turned upwards, the gravitating power of the sun, by which their ascent is perpetually retarded, be diminished by one half, they will now ascend perpetually, and all of them at all equal distances from the sun will be equally swift. *Mercury*, when he arrives at the orb of *Venus*, will be as swift as *Venus*; and he and *Venus*, when they arrive at the orb of the Earth, will be as swift as the Earth; and so of the rest. If they begin all of them to ascend at once, and ascend in the same line, they will constantly, in ascending, become nearer and nearer together, and their motions will constantly approach to an equality, and become at length slower than any motion assignable. Suppose, therefore, that they ascended till they were almost contiguous, and their motions inconsiderably little, and that all their motions were at the same moment of time turned back again; or, which comes almost to the same thing, that they were only deprived of their motions, and let fall at that time, they would all at once arrive at their several orbs, each with the velocity it had at first; and if their motions were then turned sideways, and at the same time the

the gravitating power of the sun doubled, that it might be strong enough to retain them in their orbs, they would revolve in them as before their ascent. But if the gravitating power of the sun was not doubled, they would go away from their orbs into the highest heavens in parabolical lines. These things follow from my *Princip. Math. Lib. I. Prop. xxxiii, xxxix, xxxv, xxxvi.*

I thank you very kindly for your designed present, and rest yours, &c.

L E T T E R IV.

S I R,

Cambridge,
Feb 11, 1693.

THE hypothesis of deriving the frame of the world, by mechanical principles, from matter evenly spread through the heavens, being inconsistent with my system, I had considered it very little before your letters put me upon it, and therefore trouble you with a line or two more about it, if this comes not too late for your use.

In my former I represented, that the diurnal rotations of the planets could not be derived from gravity, but required a Divine Arm to impress them. And though gravity might give the planets a motion of descent towards the sun, either directly or with some little obliquity, yet the transverse motions by which they revolve in their several orbs, required the Divine Arm to impress them according to the tangents of their orbs. I would now add, that the hypothesis of matter's being at first evenly spread through the heavens, is, in my opinion, inconsistent with the hypothesis of innate gravity, without a supernatural power to reconcile them; and therefore it infers a Deity. For if there be innate gravity, it is impossible now for the matter of the Earth and all the planets and stars to fly up from them, and become evenly spread throughout all the heavens, without a supernatural power; and certainly that which can never be hereafter without a supernatural power, could never be heretofore without the same power.

VOL. IV.

L 11

You

You queried whether matter, evenly spread throughout a finite space, of some other figure than spherical, would not, in falling down towards a central body, cause that body to be of the same figure with the whole space; and I answered, yes. But in my answer it is to be supposed that the matter descends directly downwards to that body, and that that body has no diurnal rotation.

This, Sir, is all I would add to my former letters.

I am yours, &c.

C O M M E R C I U M
E P I S T O L I C U M

D E

V A R I A R E M A T H E M A T I C A,

I N T E R

C E L E B E R R I M O S P R Æ S E N T I S S E C U L I M A T H E M A T I C O S.

V I Z.

Isaacum Newtonum Equitem Auratum.
D. Isaacum Barrow.
D. Jacobum Gregorium.
D. Johannem Wallisium.
D. J. Keillium.

D. J. Collinium.
D. Gulielmum Leibnitium.
D. Henricum Oldenbourgum.
D. Franciscum Slusium.

E T A L I O S.

J U S S U S O C I E T A T I S R E G I Æ I N L U C E M E D I T U M

E T J A M

Una cum Recensione præmissa insignis Controversiæ inter Leibnitium & Keillium de primo Inventore Methodi Fluxionum; & Judicio primarii, ut ferebatur, Mathematici subjuncto, iterum impressum. A. D. 1725.

A D

L E C T O R E M.

CUM primum *Commercium Epistolicum* lucem vidit, D. Leibnitius Viennæ agens, ut librum sine responso dimitteret, prætendit per biennium se eundem non vidiſſe, ſed ad iudicium primarii Mathematici, & barum rerum peritiſſimi, & à partium Audio alieni ſe provocâſſe. Et ſententiam ejus, 7 Jun. 1713, datam, ſchedule volanti 29 Julii datæ incluſam, per orbem ſparſit, ſine nomine vel Judicis, vel Impreſſoris, vel Urbis in quâ impreſſa fuit. Et ſub finem anni 1715, in Literis quas ad Abbatem de Comitibus tunc Londini agentem ſcripſit, confugit ad Quæſtiones novas de Qualitatibus occultis, gravitate univerſali, Miraculis, Organis & Sensorio Dei, ſpatio, Tempore, Vacuo, Atomis, Perfectione mundi, & Intelligentiâ ſupramundandâ; & Problema ex Actis Eruditorum deſumptum propoſuit ab Analyſtis Anglis ſolvendum. Quæ omnia ad rem nihil ſpectant.

Sed & Conſeſſum à Regiâ Societate conſtitutum, qui *Commercium* ex antiquis monumentis ediderant, accuſavit, quaſi partibus ſtudiſſent; & literas antiquas edendo, omiſſiſſent omnia quæ vel pro ipſo, vel contra Newtonum facerent. Et ut hoc probaret, ſcripſit is in primâ ſuâ ad Abbatem epiſtolâ, quod in ſecundo ſuo in Angliam itinere, Collinius oſtenderit ipſi partem *Commercii* ſui; in quâ Newtonus agnoſcebat ignorantiam ſuam in pluribus, dicebatque

(inter alia) quòd nihil invenisset circa dimensiones Curvilinearum, quæ celebrantur, præter dimensionem Cissoidis: sed Confessus hoc totum suppressit. Et Newtonus, in Epistolâ suâ ad dictum Abbatem 26 Feb. 17 $\frac{1}{2}$ datâ, respondit, hoc non fuisse omissum, sed extare in Epistolâ suâ ad Oldenburgum 24 Octob. 1676 missâ, & impressum fuisse in Commercio Epistolico pag. 74, lin. 10, 11. Et subinde Leibnitius, in proximâ suâ ad Abbatem illum Epistolâ Apr. 9, 1719 datâ, agnovit se errasse. Sed, inquit, exemplum dabo aliud. Newtonus in unâ Epistolarum ejus ad Collinium agnovit, se non posse invenire magnitudinem sectionum secundarum (vel segmentorum secundorum) sphæroidum & corporum similium: sed Confessus hunc locum, vel hanc Epistolam, minimè edidit. Newtonus autem, in Observationibus quas in hanc Leibnitii Epistolam scripsit, respondit: si Confessus hoc omississet, rectè omnino omissum fuisse, cum hujusmodi cavillationes ad Quaestionem, de quâ agitur, nil spectent: sed Confessum hoc minimè omississe. Collinius, in Epistolâ ad D. Gregorium 24 Decem. 1670, ut & in alterâ ad D. Bertet 1671 (utrisque impressis in Commercio, p. 24, 26) scripsit quòd Methodus Newtoni se extenderet ad secunda Solidorum segmenta, quæ per rotationem generantur. Et Oldenburgius idem scripsit ad Leibnitium ipsum 8 Dec. 1674, ut videre est in Commercio, pag. 39. Leibnitius igitur iterum erravit. Nam & in Transactionibus Philosophicis pro Jan. & Feb. 1718, pag. 925, dicitur quòd Abbas de Comitibus per horas aliquot inspexit Epistolas antiquas, & Libros Epistolarum in Archivis R. Societatis affervatos, ut aliquid inveniret, quod vel pro Leibnitio, vel contra Newtonum faceret, & in Commercio Epistolico omissum fuisset; sed ejus generis nihil invenire potuit.

Insuper D. Leibnitius, ut Commercio Epistolico sine responso dimitteret, in primâ suâ ad Abbatem de Comitibus Epistolâ dixit, eos, qui contra ipsum scripsissent (id est Confessum à Regia Societate constitutum) candorem ejus aggressos esse per interpretationes duras & male fundatas, & voluptatem non habituros esse videndi responsa ejus ad pusillas rationes eorum, qui iis tam malè utuntur. Interpretationes ille nullius quidem sunt auctoritatis, nisi quam ab Epistolis derivant; sed male fundatas esse Leibnitius nunquam ostendit.

Subinde

Subinde vero Newtonus, qui ægre adductus est ut scriberet, in primâ suâ ad Abbatem Epistolâ 26 Feb. 171 $\frac{1}{2}$ ita rescripsit. D. Leibnitius hætenus respondere recusavit, bene intelligens impossibile esse res factas refutare. Silentium suum hæc in re excusat, prætexens se librum nondum vidisse, & otium illi non esse ad examinandum, sed se orasse Mathematicum celebrem, ut hoc negotium in se fusciperet.—Utitur & novo prætextu ne respondeat, dicens quòd Angli voluptatem non habebunt videndi responsa ejus ad pusillas eorum rationes, & proponens disputationes novas Philosophicas ineundas & problemata solvenda: quæ duo ad rem nil spectant. D. Leibnitius autem, in proximâ suâ ad Abbatem Epistolâ 9 Apr. 1716 datâ, pergebat se excusare ne respondeat. Ut operi, inquit, contra me edito sigillatim respondeam, opus erit alio opere non minore quàm hoc est; percurrendum erit corpus magnum minutorum ante annos 30 vel 40 præteritorum, quorum perparvum reminiscor; examinandæ erunt veteres epistolæ, quarum plures sunt perditæ; præterquam quòd maximâ ex parte non conservavi minuta meorum, & reliquæ sepultæ sunt in maximo chartarum acervo, quem non possum sine tempore & patientiâ discutere. Sed otium mihi minimè suppetit aliis negotiis alterius prorsus generis occupato. Et paulo post: literas truncare non debuerunt. Nam parvum est inter chartas meas, vel cujus minuta mihi relinquuntur. Sic omnibus perpensis, videns tantas malignitatis & fallaciæ notas, credidi indignum esse me ingredi discussionem cum hominum genere, qui se tam malè gerunt. Sentio quòd in iis refutandis difficile fuerit ab opprobriis & expressionibus asperis abstinere, talibus quas eorum facta merentur; & non cupio hujusmodi spectaculum exhibere publico, in animo habens tempus meum melius impendere, quod mihi pretiosum esse debet, & contemnens judicium eorum qui super tali opere sententiam contra me pronunciare vellent; præsertim cum Societas Regia hoc facere noluit. Hæc Leibnitius. Quaestionem primam deserit rixando, & Quaestiones novas proponit.

Attamen post ejus mortem, quæ contigit proximo Mense Novembris, in Elogio ejus quod in Actis Eruditorum pro Mense Julio anni 1717 impressum fuit, amici ejus scripserunt eum Commercio Epistolico Anglorum quoddam suum idemque amplius opponere de-
crevisse:

crevisse: & paucis ante obitum diebus Cl. *Wolffio* significasse se Anglos, famam ipsius laceffentes, reipsa refutaturum. Quamprimum enim à laboribus historicis vacaturus sit, daturum se aliquid in Analyfi prorsus inexpectatum, & cum inventis quæ hactenus in publicum prostant, sive *Newtoni*, sive aliorum, nihil quicquam affine habens. *Hæc illi. Verum ex jam dictis patet, illum non aliud habuisse commercium Epistolicum, quod ederet. Et inventum novum, bis nihil affine habens, ad rem nihil spectat. Missis ægrorum somniis, Quæstio tota ad epistolas antiquas referri debet.*

Initio secundæ ad Abbatem de Comitibus Epistolæ, D. Leibnitius primam *Newtoni Epistolam* vocavit speciem chartæ provocatoriæ ex parte *Newtoni*: dein addidit; in arenam descendere nolui contra ejus milites emissarios, sive intelligas accusatorem supra fundamentum *Commercii Epistolici*, sive præfationem spectes acrimoniæ plenam, quam alius quidam novæ Principiorum editioni præmisit: sed cum is per se jam lubens apparebit, paratus sum ipsi satisfactionem dare. Et *Newtonus respondit*, D. Leibnitium literas & chartas antiquas seponere, & ad quæstiones circa philosophiam & res alias confugere. Et magnum illum Mathematicum, cui sine nomine, ut judici, epistolam 7 Jun. 1713 datam attribuerat, jam velo sublato ut militem in hac rixâ pro se inducere, mathematicos in Anglia provocantem ad problemata solvenda; quasi duellum [*cum Leibnitio scilicet*] vel fortè prælium cum exercitu discipulorum ejus [*quos jactat*] methodus esset magis idonea ad veritatem dirimendam, quam discussio veterum & authenticorum scriptorum, & Mathesis factis heroicis, vice rationum ac demonstrationum, abhinc implenda esset. *Hic rationes ac demonstrationes alludunt ad argumenta è scriptis veteribus desumpta, & facta heroica ad contentiones philosophicas & problemáticas ad rem nil spectantes, ad quas D. Leibnitius à prioribus aufugit.*

Quæ novæ Principiorum editioni præmissa sunt, *Newtonus non vidit, antequam Liber in lucem prodiret. Quæ de Quæstionibus Philosophicis disputata sunt, D. Des Maizeaux collegit, & in lucem edidit. Solutiones Problematum maximâ ex parte lucem viderunt in Aëlis Eruditorum. Hæc omnia ad rem nil spectant. Commercii Epistolici exempla tantum pauca impressa fuerunt, & ad Mathematicos missa qui de his rebus judicare possent, neque prostant venalia.* Ideoque

que hunc Librum, ut & ejus Recensionem quæ in Transactionibus Philosophicis ac Diario Literario, anno 1715, anno & septem vel octo mensibus ante obitum D. Leibnitii, impressa fuit, iterum imprimere visum est; ut historia vera ex antiquis monumentis deducta, missis disputationibus quæ ad rem nil spectant, ad posteros perveniat; & sic finis imponatur huic controversiæ. Nam D. Leibnitius à Quæstione desciscens emortuus est, & judicium posteris relinquitur.

Denique Judicium primarii Mathematici subjunctum est, unâ cum notis; quibus pateat, eidem in Recensione prædictâ, vivente Leibnitio, responsum esse, & scopum ejus fuisse tantum, ut *Commercium Epistolicum sine responso dimitteretur.*

R E C E N S I O L I B R I

Qui inscriptus est *Commercium Epistolicum Collinii & aliorum de Analyfi Promotâ*, & publicatus est jussu Regiæ Societatis Londinensis, circa controversiam inter D. Leibnitz & D. Keill, de primo inventore *Methodi Fluxionum*, five, ut nonnulli appellant, *Methodi Differentialis*: Anglicè primùm edita in Actis Regiæ Societatis, A. D. 1715, & Gallicè eodem anno in Diario Literario Tom. VII. nunc ex Anglico in Latinum versa.

CUM variæ Relationes apud externos de Commercio hoc publicatæ sint, mutilæ omnes & imperfectæ; visum est ut plenior hæc, quæ sequitur, Recensio in publicum edatur.

Commercium hoc contextum est ex variis epistolis chartisque, in archivis Regiæ Societatis repositis; quæ singulæ hîc suo ordine ac serie collocantur, & vel ex latinis fideliter transcriptæ sunt, vel ex anglicis fideliter in latinum translatae: numerofo Confessu à Regiâ Societate deputato, ut & literæ originales inspicerentur, & earum exemplaria examinarentur. Cæterum hæc, de quâ agitur, est methodus generalis resolvendi finitas æquationes in infinitas, & applicandi æquationes illas, tam finitas quàm infinitas, ad solutionem problematum, per methodum *Fluxionum* & *Momentorum*. Primò autem differemus de eâ Methodi parte, quæ consistit in resolvendo finitas æquationes in infinitas, & eâ ratione quadrando figuras curvilineas. Per infinitas æquationes intelliguntur illæ, quæ involvunt seriem terminorum convergentium, & ad veritatem propius propiusque accedentium in infinitum; ita ut postremò à veritate distent minùs ulla datâ quantitate; &, si in infinitum continuentur, nullam omnino differentiam relinquant.

Wallisius

C O M M E R C I I E P I S T O L I C I.

Wallisius in opere suo arithmetico, publicato A. D. 1657. Cap. 33. Prop. LXVIII. reduxit refractionem $\frac{A}{1-R}$, per perpetuam divisionem, in seriem $A + AR + AR^2 + AR^3 + AR^4 + \&c.$

Vicecomes Brounker quadravit hyperbolam per hanc seriem $\frac{1}{1x} + \frac{1}{3x^3} + \frac{1}{5x^5} + \frac{1}{7x^7} + \&c.$ hoc est per hanc $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} + \&c.$ conjungendo singulos binos terminos in unum. Et hæc quadratura publicata est in Actis Regiæ Societatis, mense Aprili 1668.

Paulo post Dominus Mercator evulgavit demonstrationem hujus quadraturæ per divisionem Domini Wallisii; & deinceps haud multo post Jacobus Gregorius geometricam ejusdem demonstrationem in lucem edidit. Hi libelli, paucis postquam editi sunt mensibus, Cantabrigiam missi sunt ad Dominum Barrovium per Dominum Johannem Collins; & per Barrovium traditi Isaaco Newtono tunc Cantabrigiæ degenti, utpote Collegii S. Trinitatis Socio, (nunc autem Londini Equiti Aurato) mense Junio 1669. Hæc occasione, Barrovius vicissim Collinio misit tractatum Newtoni, qui inscribebatur *Analysis per æquationes numero terminorum infinitas*. Commerc. Epist. N° I. Is tractatus in Commercio Epistolico agmen ducit, continetque universalem methodum id in omnibus figuris faciendi, quod vicecomes Brounker & Mercator in solâ hyperbolâ fecerant. Porro Mercator, per annos sexdecim adhuc superstes, nihil tentavit aut progressus est ultra solam illam hyperbolæ quadraturam. Illa verò Newtoni per omnes figuras progressio satis ostendit, nihil eum in eâ re Mercatoris operâ aut ope indiguissè. Ne tamen litiget quisquam, aut cavilletur; concedit Newtonus & Brounkerum invenisse, & Mercatorem demonstrâsse, seriem illam pro hyperbolâ quadrandâ, annos prius aliquot quàm in publicum ederent; & proinde prius quàm Newtonus generalem suam methodum invenisset.

De tractatu isto qui inscribitur *Analyfis*, &c. Newtonus in epistolâ ad Oldenburgium missâ, datâque 24 Octob. 1676, hæc verba ib. N° LVII. habet, quæ sequuntur: ‘*Eo ipso tempore quo Mercatoris Logarithmotecnia prodiit, communicatum est per amicum D. Barrow (tunc Matheseos professorem Cantab.) cum D. Collinio compendium quoddam harum serierum, in quo significaveram areas & longitudines Curvarum omnium, & Solidorum superficies & contenta*

M m m 2

‘ ex

‘*ex datis rectis; & vice versa, ex his datis rectas determinari posse;*
 ‘*& methodum indicatam illustraveram diversis seriebus.*’ Hujus
 porro serierum compendii certiores fecit Collinius Jacobum Grego-
 rium Scotum, Dominos Bertet & Vernon apud Gallos, Alphonsum
 Borellum Italum, Dominos Strode, Townsend, Oldenburg, Dary,
 aliosque apud Anglos, variis epistolis datis ann. 1669, 1670,

Fr. N° XIV, 1671 & 1672, ut ipsæ epistolæ adhuc testantur. Ipse præterea
 XIX. XXI.
 XXII. XXIII.
 XXIV.
 Ib. N° XIII. Slusio, Leodii tum agente, & ex eâ aliquot *ῥήσεις* citavit; literis

dati 14 Sept. 1669, & in librum Regiæ Soc. epistolarem tran-
 scriptis. Porro Collinius in epistolâ ad Jac. Gregorium, 25
 Ib. N° XIV. Novemb. 1669, sic de methodo in *Analyfi* illâ contentâ loquitur.

‘Barrovius provinciam suam publicè prælegendi remisit cui-
 ‘dam nomine Newtono Cantabrigiensi; cujus, tanquam viri acu-
 ‘tissimo ingenio præditi, in præfatione Prælectionum Opticarum
 ‘meminit. Quippe antequam ederetur Mercatoris Logarithmo-
 ‘technia, eandem methodum adinvenerat, eamque ad omnes
 ‘Curvas generaliter, & ad circulum diversimodè applicarat.’ Lite-

Ib. N°
 VII.

ris verò ad D. Davidem Gregorium datis 11 Aug. 1676, his verbis
 de eâ loquitur: ‘Paucos post menses quàm editi sunt hi libri (viz.
 ‘Mercatoris Logarithmotechnia, & Exercitationes Geometricæ Gre-
 ‘gorii) missi sunt ad Barrovium Cantabrigiæ. Ille autem re-
 ‘sponsum dedit, hanc infinitarum serierum doctrinam à Newtono
 ‘biennium ante, excogitatam fuisse, quàm ederetur Mercatoris Lo-
 ‘garithmotechnia, & generaliter omnibus figuris applicatam; si-
 ‘mulque transmisit D. Newtoni opus manuscriptum.’ Horum
 autem librorum posterior prodiit circa finem anni 1668; Barro-

Ib. N° I.

vius verò dictarum serierum compendium Collinio misit, Julio inse-
 Ib. N° XXIV. quente; ut ex tribus ejus epistolis constat. Collinius porro, in li-
 teris ad D. Strode 26 Julii 1672, sic de eo compendio scribit:
 ‘Exemplar ejus (Logarithmotechniæ) misi Barrovio Cantabrigiam,
 ‘qui quasdam Newtoni chartas extemplo remisit; è quibus &
 ‘aliis, quæ prius ab auctore cum Barrovio communicatæ fuerant,
 ‘patet illam methodum à dicto Newtono aliquot annis antea exco-
 ‘gitatam, & modo universali applicatam fuisse. Ita ut ejus ope
 ‘in quâvis figurâ curvilinæa propositâ, quæ unâ vel pluribus pro-
 ‘prietatibus definitur, quadratura vel area dictæ figuræ, accurata

‘fi

‘si possibile sit, sin minus infinitè vero propinqua, evolutio vel
 ‘longitudo lineæ curvæ, centrum gravitatis figuræ, Solida ejus
 ‘rotatione genita, & eorum superficies, sine ullâ radicum extrac-
 ‘tione obtineri queant. Postquam intellexerat D. Gregorius hanc
 ‘methodum, à D. Mercatore in Logarithmotechniâ usurpatam, &
 ‘hyperbolæ quadrandæ adhibitam, quamque adauxerat ipse Gre-
 ‘gorius, jam universalem redditam esse, omnibusque figuris ap-
 ‘plicatam, acri studio eandem acquisivit, multumque in eâ eno-
 ‘dandâ defudavit. Uterque D. Newtonus & Gregorius in animo
 ‘habent hanc methodum exornare: D. Gregorius autem D. New-
 ‘tonum primum ejus inventorem anticipare haud integrum ducit.
 In aliâ verò epistolâ, ad Oldenburgium scriptâ & cum D. Leibnitio
 communicandâ, datâque 14 Jun. 1676, hæc memorat Collinius: Ib. N° XLV.
 ‘Hujus autem methodi ea est præstantia; ut, cum tam latè pa-
 ‘teat, ad nullam hæreat difficultatem. Gregorium autem aliof-
 ‘que in eâ fuisse opinione arbitror, ut quicquid uspiam antea de
 ‘hâc re innotuit, quasi dubia diluculi lux fuit, si cum meridianâ
 ‘claritate conferatur.’

Porro hic Newtoni tractatus primum typis editus est à D. Guli- A. D. 1710.
 elmo Jones, qui apographum ejus repperit in scriniis Collinii, ip-
 sius manu scriptum; & postea cum originali contulit, à D. New-
 tono mutuato. Continet autem prædictam generalem methodum
 Analyseos, monstrantem quomodo resolvendæ sunt finitæ æqua-
 tiones in infinitas; utque, per methodum momentorum, applican-
 dæ sunt æquationes, tam finitæ quàm infinitæ, ad omnium proble-
 matum solutionem. Incipit verò, ubi finem fecit Wallisius, &
 methodum quadraturarum super tres regulas struit.

Wallisius anno 1655, *Arithmeticam suam Infinitorum* in lucem
 dedit; per ejus libri Prop. LIX. si abscissa cujusvis curvilinearis
 figuræ vocatur x , & n atque m sint numeri, & ordinatæ ad rectos

angulos erectæ sint x^n ; area figuræ erit $\frac{n}{m+n} x^{\frac{m+n}{n}}$. Atque hoc Ib. N° II.

assumitur à D. Newtono, tanquam prima regula, super quam
 fundat suam Curvarum quadraturam. Wallisius autem proposi-
 tionem hanc demonstravit gradatim, per multas particulares pro-
 positiones; tandemque omnes in unam collegit per tabulam ca-
 suum. Newtonus verò omnes casus in unum reduxit, per digni-
 tatem

tatem cum indefinito indice: & sub extremo Compendii, semel simulque demonstravit per methodum suam momentorum; primusque indefinitos dignitatum indices in operationes analyticos introduxit.

Cæterum per CVIII Propositionem *Arithmeticæ Infinitorum* Wallisii, perque plures alias propositiones quæ sequuntur; si Ordinata composita fuerit ex duabus vel pluribus ordinatis cum signis suis + & - acceptis, Area composita erit ex duabus vel pluribus areis cum signis suis + & - acceptis respectivè. Atque hoc à D. Newtono assumitur, tamquam regula secunda, super quam instituit suam quadraturarum methodum.

Ib. N° III.

Ib. N° IV.

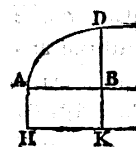
Tertia verò regula est, ut reducantur fractiones & radicales, & affectæ radices æquationum in series convergentes, cum quadratura non aliter succedat: & ut per regulas primam ac secundam quadrentur figuræ, quarum Ordinatæ sunt singuli termini serierum. Newtonus, in epistolâ ad Oldenburgium scriptâ 13 Jun. 1676, & Leibnitio transmissâ, modum docuit reducendi quamlibet dignitatem cujuslibet binominalis in seriem convergentem; & per eam seriem quadrandi Curvam, cujus Ordinata est illa dignitas. Et à D. Leibnitio rogatus, ut fontem hujus theorematis explicare vellet, rescripsit per epistolam datam 24 Octob. 1676; se paulo ante pestem, quæ Londini grassabatur anno 1665, cum legeret Arithmeticam Infinitorum Wallisii, cogitaretque de interpolandâ serie $x, x - \frac{1}{3}x^3, x - \frac{2}{5}x^5 + \frac{1}{7}x^7, x - \frac{3}{5}x^5 + \frac{3}{7}x^7 - \frac{1}{9}x^9, \&c.$ invenisse aream circuli esse $x - \frac{1}{3}x^3 - \frac{1}{5}x^5 - \frac{1}{7}x^7 - \frac{1}{9}x^9 - \&c.$ & persequendo methodum interpolationis, se prædictum theorema excogitasse; atque ejus ope reductionem fractionum & surdarum in series convergentes, per divisionem & radicum extractionem, invenisse; ac tum ad affectarum radicum extractionem perrexisse. Atque hæ reductiones regula sunt tertia.

Ib. N° IV.

Ib. N° X.

Cum in hoc serierum compendio trinas has regulas explicuisset Newtonus, variisque exemplis eas illustrasset; designavit is ideam deducendi aream ex ordinatâ, considerando aream tamquam quantitatem nascentem, & augescentem sive crescentem per fluxionem continuam, in proportionem longitudinis ordinatæ, & supponendo abscissam uniformiter crescere in proportionem ad tempus. Atque

Atque ex momentis temporis, nomen *Momentorum* indidit momentaneis augmentis, sive partibus areæ atque abscissæ, infinitè parvis, quæ in momentis temporis generantur. Momentum lineæ punctum vocavit, ex mente Cavallerii; quamvis non sit punctum geometricum, sed lineola infinitè brevis: momentum autem areæ vel superficiæ vocavit lineam, secundum eundem Cavallerium; licet non sit linea geometrica, sed superficies latitudine infinitè exili. Cumque Ordinatam consideraret tanquam momentum areæ, eo nomine intellexit rectangulos sub geometricâ Ordinatâ & momento abscissæ; licet illud momentum non semper exprimatur. Sit ABD, inquit, *Curva quævis*, \odot AHKB



rectangulum, cujus latus AH, vel KB, est unitas. Et cogita rectam DBK, uniformiter ab AH, motam areas ABD \odot AK describere: \odot quod [recta] BK (1) sit momentum quo [area] AK (x), \odot [recta] BD (y) momentum quo [curvilinea] ABD gradatim augetur: \odot

quod ex momento BD, perpetim dato, possis per præcedentes [tres] regulas aream ABD, ipso descriptam, investigare; sive cum areâ AK (x), momento 1 descriptâ, conferre. Hæc Newtoni idea est operationis in curvis quadrandis: quoque modo hanc ad alia problemata applicet, in verbis proxime sequentibus monstrat: jam, inquit, quâ ratione superficies ABD, ex momento suo perpetim dato, per præcedentes [tres] regulas elicitur, eadem quælibet alia quantitas ex momento suo sic dato elicitur. Exemplo res fiet clarior. Cæterum post aliquot exempla, methodum addit regressionis ab areâ, arcu, solidove contento ad abscissam; docetque ut eadem methodus extendat se ad Curvas mechanicas, determinando earum ordinatas, tangentes, areas, longitudines, &c. Utque assumendo quamvis æquationem exprimentem relationem inter aream abscissamque Curvæ, per hanc methodum invenias Ordinatam. Atque hoc est fundamentum methodi fluxionum & momentorum, quod Newtonus, in literis datis 24 Octob. 1676, hæc sententiâ comprehendit: *Datâ æquatione quocunque fluentes quantitates involvente, invenire fluxiones; \odot vice versâ.*

In hoc Compendio uniformem fluxionem temporis, vel cujusvis exponentis temporis, per unitatem representat Newtonus; momentum autem temporis, vel exponentis sui, per literam o; fluxiones vero

Ib. N° X,
XII.

verò aliarum quantitatum, per quævis alia symbola; ac momenta earum quantitatum, per rectangulos sub illis symbolis & litera o ; aream porro Curvarum, per ordinatam in quadrato inclusam; areâ pro fluente, & ordinatâ pro ejus fluxione positâ. Cum autem propositionem aliquam demonstrat, literam o adhibet pro finito momento temporis vel ejus exponentis, aut cujusvis quantitatis uniformiter fluentis; totamque calculationem absolvit per geometriam veterum, in finitis figuris five schematibus, sine ullâ approximatione: & cum primum calculatio peracta est, & æquatio reducta, supponit momentum o decrescere in infinitum, atque evanescere. Cum verò non demonstrat, sed solum investigat propositionem; quo citius rem conficiat, supponit momentum o esse infinitè parvum, & in scribendo illud negligit, omnibusque approximationum modis utitur, quos nullum in conclusione errorem parituros autumat. Prioris generis specimen habes sub fine Compendii, ubi primam trium illarum regularum, quas initio libri posuerat, erat demonstraturus. Secundi generis exempla ibidem habes; cum invenit curvarum linearum longitudinem p . 15*; & cum eruit ordinatas, areas, & longitudes Curvarum mechanicarum p . 18, 19†: narratque, quâ viâ per eandem methodum tangentes duci possint ad Curvas mechanicas, p . 19‡. Atque

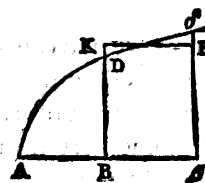
Ib. N° XII.

Ib. N°XXVI. in epistolâ datâ 10 Decemb. 1672, addit, problemata de curvaturâ Curvarum, seu geometricarum five mechanicarum, per eandem methodum solvi. Ex quibus manifestum est, se jam tum suam methodum ad secunda ac tertia momenta extendisse: cum enim areæ Curvarum considerantur tamquam fluentes, ut in hac Analyfi fieri solet, ordinatæ exprimunt fluxiones primas; tangentes autem datæ sunt per fluxiones secundas, & curvaturæ per tertias. Et vel in Analyfi hac p . 16 §. ubi Newtonus ait, *Momentum est superficies, cum de Solidis*; & *linea, cum de superficibus*; & *punctum, cum de lineis agitur*; perinde est ac si dixisset, cum Solida considerantur tamquam fluentia, eorum momenta superficies sunt; & eorum momentorum momenta, vel secunda momenta, lineæ sunt; & horum momentorum momenta, five tertia momenta, puncta sunt, secundum sententiam Cavallerii: atque in *Principiis* suis *Philosophiæ*, ubi frequenter considerat lineas tamquam fluentes, à punctis descriptas, quorum velocitates crescunt vel decrescunt,

* Tom. I. p. 275. † Tom. I. p. 278, 279. ‡ Tom. I. p. 279. § Tom. I. p. 276. velocitates

velocitates sunt fluxiones primæ; & earum incrementa, secundæ. Ac problema illud, *Datâ æquatione fluentes quantitates involvente, fluxiones invenire*, & vice versâ, ad fluxiones omnes pertinet; ut constat ex solutionis ejus exemplis à Wallisio publicatis, Tom. II. operum suorum, p. 391, 392, 396. Quin & in Lib. II. *Principiorum Philosophiæ*, Prop. XIV. differentiam secundam Newtonus appellat differentiam momentorum.

Quoque melius intelligas, quo calculationis genere Newtonus usus fuerit anno 1669, vel ante, cum hoc analyseos suæ compendium scripsit; ponam hic ejus demonstrationem primæ illius regulæ, supra memoratæ.



‘ Sit Curvæ alicujus, ADD , basis $AB=x$; perpendiculariter applicata $BD=y$; & area ABD $=z$, ut prius. Item sit $B\beta=o$, $BK=v$, & rectangulum $B\beta HK$ (ov) æquale spatio $B\beta\delta D$. Est ergo $A\beta=x+o$, & $A\delta\beta=z+ov$. His præmissis, ex relatione inter x & z , ad arbitrium assumptâ, quæro y ; ut sequitur. Pro

‘ lubitu sumatur [æquatio] $\frac{2}{3}x^3=z$, five $\frac{4}{3}x^3=2z$. Tum $x+o$ ($A\beta$) pro x , & $z+ov$ ($A\delta\beta$) pro z substitutis, prodibit $\frac{4}{3}$ in $x^3 + 3xxo + 3xoo + o^3 =$ (ex naturâ Curvæ) $z^2 + 2zov + o^2v^2$. Et sublatis $\frac{4}{3}x^3$ & $2z$ æqualibus, reliquisque per o divis, restabit $\frac{4}{3}$ in $3x^2 + 3xo + o^2 = 2zv + ov^2$. Si jam supponamus $B\beta$ in infinitum dimini, & evanescere, five o esse nihil; erunt v & y æquales, & termini per o multiplicati evanescunt; ideoque restabit $\frac{4}{3} \times 3xx = 2zv$, five $\frac{2}{3}xx (=zy) = \frac{2}{3}x^2y$, five $x^2 (= \frac{x^3}{x}) = y$. Quare contra, si $x^2=y$, erit $\frac{2}{3}x^3=z$.

‘ Vel generaliter, si $\frac{n}{m+n} \times ax^{\frac{m+n}{n}} = z$: five ponendo $\frac{na}{m+n} = c$, & $m+n=p$; si $cx^{\frac{p}{n}} = z$, five $c^n x^p = z^n$; tum $x+o$ pro x , & $z+ov$ (five quod perinde est $z+oy$) pro z substitutis, prodit c^n in $x^p + pox^{p-1}$, &c. $= z^n + noyz^{n-1}$, &c. reliquis nempe [serierum] terminis, qui tandem evanescerent, ommissis. Jam sublatis $c^n x^p$ & z^n æqualibus, reliquisque per o divis, restat $c^n px^{p-1} = nyz^{n-1} = \frac{nyz^n}{z}$ $= \frac{nyc^p x^p}{cx^{\frac{p}{n}}}$; five dividendo per $c^n x^p$, erit $px^{p-1} = \frac{ny}{c^n}$; five

VOL. IV.

N n n

‘ pcx

' $pcx^{\frac{m-n}{n}} = ny$; vel restituendo $\frac{na}{m+n}$ pro c , & $m+n$ pro p , hoc est
' m pro $p-n$, & na pro pc , fiet $ax^{\frac{m}{n}} = y$. Quare è contra si $ax^{\frac{m}{n}} = y$,
'erit $\frac{n}{m+n} ax^{\frac{m+n}{n}} = z$. Q. E. D.'

Eàdem operandi ratione, etiam regula secunda demonstrari potest. Et si quælibet æquatio assumatur, relationem exprimens inter abscissam & aream; ordinata inveniri poterit eàdem ratione; ut in proximis Analyseos verbis indicatur. Et si illa ordinata, in unitatem ducta, pro aieà novæ Curvæ ponatur, novæ illius Curvæ ordinata eàdem methodo inveniri potest; atque ita in perpetuum. Hæque ordinatæ primam, secundam, tertiam, quartam, sequentesque fluxiones primæ areæ repræsentant.

Hæc Newtoniana fuit operandi methodus, eo tempore quo compendium illud suæ Analyseos scripsit: eàdemque methodo usus est in libro Quadraturarum, atque in hunc usque diem adhuc utitur.

In exemplis, quibus methodum serierum & momentorum in Compendio hoc illustrat, hæc sunt: Esto radius circuli 1; arcus, z ; sinus, x ; æquationes pro inveniendi arcu, cujus sinus est datus, & sinu, cujus arcus est datus, erunt

Nº X.

$$z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{1152}x^9 + \&c.$$

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{36288}z^9 - \&c.$$

Nº XIV,
XIX, XX.

Hujus methodi notitiam Collinius Gregorio dedit sub autumnno anni 1669; ac Gregorius, ope unius ex seriebus Newtonianis, post integrum annum laborem, methodum demum invenit *Decemb.* 1670: & bimestri post tempore, in epistolâ datâ 15 *Feb.* 1671, varia theoremata, per eam reperta, Collinio misit; datâ etiam communicandi licentiâ. Collinius autem facillimus erat ad communicandum quæcunque vel à Newtono vel Gregorio accepisset; ut patet ex epistolis in hoc Commercio jam publicatis. In seriebus, quas in dictâ epistolâ misit Gregorius, hæc duæ sunt: Esto radius circuli r ; arcus, a ; & tangens, t : æquationes, pro inveniendi arcu cujus tangens data est, & tangente cujus arcus datus est, erunt hæc,

$$a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.$$

$$t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \&c.$$

Eo ipso anno 1671 D. Leibnitijs duos tractatus edidit Londini; unum Societati Regiæ, alterum Academiæ Scientiarum *Parisiensi* dedicatum; & in prioris dedicatione commercium suum epistolare cum D. Oldenburgio memorat.

Mense *Feb.* 1671, cum, in ædibus D. Boyle, Leibnitijs in D. Pel. Nº xxx. lium incidisset, sibi arrogare visus est differentialem methodum Moutoni; cumque Pellius ostendisset methodum illam non novam esse, sed Moutoni, perstitit nihilominus Leibnitijs in vindicandâ sibi inventionem illâ; præ se ferens, suo se Marte invenisse, Moutoniani operis ignarum, multumque eam promovisse.

Cum Newtoni serierum una Gregorio missa esset, tentavit ille eam deducere ex seriebus suis, inter se combinatis; ut ipse in epistolâ narrat datâ 19 *Decemb.* 1670. Ac per similem aliquam methodum Leibnitijs, priusquam Londino discederet, repperisse videtur summam seriei fractionum decreescentium in infinitum, Nº xxx. quarum numerator est numerus datus, denominatores autem sunt triangulares, vel pyramidales, vel triangulo-triangulares, &c. Ingens verò mysterium! De serie $\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \frac{1}{4} + \frac{1}{5} + \&c.$ subduc omnes terminos præter primum, & remanebit 1 ($= 1 - \frac{1}{2} + \frac{1}{2} - \frac{1}{3} + \frac{1}{3} - \frac{1}{4} + \frac{1}{4} - \frac{1}{5} + \frac{1}{5} - \&c.$) $= \frac{1}{1 \times 2} + \frac{1}{2 \times 3} + \frac{1}{3 \times 4} + \frac{1}{4 \times 5} + \&c.$ Et ab hac serie deme omnes terminos, excepto primo, & remanebit $\frac{1}{2} = \frac{2}{1 \times 2 \times 3} + \frac{2}{2 \times 3 \times 4} + \frac{2}{3 \times 4 \times 5} + \frac{2}{4 \times 5 \times 6} + \&c.$ Et à priore serie deduc omnes terminos præter duos primos, & remanebit $\frac{1}{3} = \frac{2}{1 \times 3} + \frac{2}{2 \times 4} + \frac{2}{3 \times 5} + \frac{2}{4 \times 6} + \&c.$

Sub finem *Febr.* vel initium *Martii*, 1671, Leibnitijs, Londino Nº xxxi. relicto, Parisios se contulit; & ad usque mensem Junium sequentem commercium cum Oldenburgio habuit: deinde Algebram & Geometriam sublimiorem didicit, & mense *Julio* anni 1674, Nº xxxii. commercium cum Oldenburgio renovavit; scribens se mirificum habere theorema, quod daret circuli, vel ejus sectoris cujuscunque, aream accuratè in serie numerorum rationalium; *Octobri* autem insequente scripsit, se invenisse circumferentiam circuli in serie simplicissimorum numerorum; & eàdem *metodo* (sic enim theorema illud nominat) quemvis arcum, cujus sinus datus sit, posse inveniri

veniri in simili serie, licet proportio ad totam circumferentiam ignoretur. Theorema ergo istud hoc efficiebat; ut inveniretur quivis sector, vel arcus, cujus sinus datus sit. Si ignota esset arcus proportio ad circumferentiam totam, theorema, sive methodus ista, tantummodo arcum exhibuit; sin nota esset, etiam integram circumferentiam dedit: & proinde erat theorema prius illud ex duobus supradictis Newtoni. Demonstratio vero hujus theorematum Leibnitio tum non innotuit. Quippe, in epistolâ datâ 12 Maii 1676, rogavit Oldenburgium, ut demonstrationem ejus à Collinio sibi pararet; eam significans methodum, per quam Newtonus id invenerat.

N° XLIV.

N° XXXVI.

N° XXXVII.

In epistolâ à Collinio scriptâ, datâque 15. Apr. 1675, Oldenburgius ad Leibnitium misit octo ex Newtonianis & Gregorianis seriis; in quibus erant duæ illæ Newtoni supradictæ, pro inveniendâ arcu, cujus sinus datus est, & sinu, cujus datus est arcus; duæque illæ Gregorii jam antè memoratæ, pro inveniendâ arcu, cujus tangens data est, & tangente cujus datus est arcus. Leibnitius verò in responso dato 20 Maii 1675, se epistolam istam accepisse his verbis confitebatur: *Literas tuas multâ fruge algebraicâ refertas accepi; pro quibus tibi & doctissimo Collinio gratias ago. Cum nunc, præter ordinarias curas, mechanicis imprimis negotiis distrabar, non potui examinare series quas misisti, ac cum meis comparare. Ubi fecero, perscribam tibi sententiam meam: nam aliquot jam anni sunt, quod inveni meas viâ quâdam sic satis singulari.*

Nunquam tamen postea, vel * agnovit Leibnitius se recepisse illas series; vel indicavit, quâ in re suæ ab illis differrent; vel unquam ullas alias protulit, præter illas ab Oldenburgio missas, aut series numerales ex eis deductas in casibus particularibus. Quid autem egerit cum Gregorii serie, pro inveniendâ arcu cujus tangens data est, ipse narrat in Actis Eruditorum mensis April. 1691, p. 178. *Jam, inquit, anno 1675, compositam habebam opusculum Quadraturæ Arithmeticæ, ab amicis ab illa tempore lectum, &c.* Per theorema pro transmutandis figuris, simile illis Barrovii & Gregorii, jam tandem invenerat seriei hujus demonstrationem; atque

* Cum hæc recensio scriberetur, non agnoverat; sed anno subsequente, in epistolâ ad Comiti-

atque id erat Opusculi istius argumentum. Nondum tamen acquisiverat cæterarum demonstrationem; & occasionem nactus hujus quoque expetendæ, sequentem epistolam Oldenburgio scripsit 12 Maii, 1676, Parisiis datam:

Cum Gregorius Mohr Danus nobis attulerit communicatam sibi N° XLIV. à doctissimo Collinio vestro expressionem rationis inter arcum & sinum, per infinitas series sequentes; posito sinu x, arcu z, radio 1,

$$z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{1152}x^9 + \&c.$$

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9 - \&c.$$

Hæc, INQUAM, cum nobis attulerit ille, quæ mihi valde ingeniosa videntur, & posterior imprimis series elegantiam quandam singularitatem habeat: ideo rem gratam mihi feceris, vir clarissime, si demonstrationem transmiseris. Habebis vicissim mea, ab his longè diversa, circa hanc rem meditata; de quibus jam aliquot abhinc annis ad te perscripsisse credo, demonstratione tamen non additâ, quam nunc polio. Oro ut Clarissimo Collinio multam à me salutem dicas: is faciliè tibi materiam suppeditabit satisfaciendi desiderio meo.

Hic, qui illud INQUAM legerit, facile existimaverit Leibnitium duas illas series nunquam antea vidisse; diversaque ejus circa hanc rem meditata prorsus aliud esse, quàm serierum unam, quas anno superiore receperat ab Oldenburgio, demonstrationemque istam, quam tunc expoliret, quantivis pretii fore; quippe quam pro Newtonianæ methodi munere ἀντίδοτον acceptissimum erat misurus.

Hæc epistolâ receptâ, Oldenburgius Colliniusque, literis ad Newtonium scriptis, vehementer operam dabant, ut ipse Newtonus methodum suam describeret, Leibnitio communicandam. Quam ob rem Newtonus epistolam scripsit, 13 Junii 1676 datam: in N° XLVIII. quâ eo modo serierum methodum descripsit, quo antea in supradicto compendio fecerat; hæc tamen differentia: hîc fusè descripsit reductionem dignitatis binomialis in seriem; at reductionem per divisionem, radicumque affectarum extractionem, leviter tantum attigit: illuc reductionem fractionum & radicalium in series, per divisionem radicumque extractionem, fusè descripsit; at posuit tantummodo duos primos terminos seriei, in quam dignitas binomialis reduci possit. Inter exempla, quæ epistola illa continet de Kilmansegg, agnovit se tunc ab Oldenburgio accepisse [Des Essais] serierum specimina.

nebat,

N° XLIX. nebat, erant series pro inveniendō numero, cujus logarithmus sit datus, & pro inveniendō verso sinu, cujus arcus datus sit. Hæc epistola Parisios missa est 26 Jun. 1676, unā cum manuscripto quodam Collinii, extracta quædam continente ex epistolis Jacobi Gregorii.

N° XLVI. Gregorius enim prope finem anni 1675 diem suum obierat; Colliniusque, exoratus à Leibnitio aliisque ex Academiâ Scientiarum, extracta ex ejus epistolis confecit; quæ adhuc extant ipsius Collinii manu exarata, hoc titulo: *Extracta ex D. Gregorii literis, D. Leibnitio commodanda, qui exorandus est, ut cum usus eis fuerit, tibi ea remittat.* Porro hæc extracta ad Leibnitium missa fuisse, testis est ipse Collinius, in epistolâ ad Davidem Gregorium, Jacobi τὸ μακρότερον fratrem, datâ 11 Aug. 1676; idque amplius constat ex Leibnitii Tschurnhausique responsis.

Leibnitii responsum, Oldenburgio missum datumque 27 Aug. 1676, sic incipit. *Literæ tuæ, die Julii 26 datæ, plura ac memorabilia circa rem analyticam continent, quàm multa volumina spissa de his rebus edita. Quare tibi pariter, ac clarissimis viris Newtono ac Collinio, gratias ago, qui nos participes tot meditationum egregiarum esse voluistis.* Et prope finem epistolæ, postquam Newtonianæ epistolæ contenta enarrasset, ita pergit: *Ad alia tuarum literarum venio, quæ doctissimus Collinius communicare gravatus non est. Vellem adjecisset appropinquationis Gregorianæ linearis demonstrationem.* Fuit enim his certè studiis promovendis amplissimus.

N° LIII. Responsum verò Tschurnhausii, datum 1 Sept. 1676, cum Newtoni de seriebus epistolam commemorasset, his verbis concluditur: *Similia porro, quæ hæc in re præstitit eximius ille geometra Gregorius, memoranda certè sunt. Et quidem optimè famæ ipsius consulturi sunt, qui, ipsius relicta manuscripta luci publicæ ut exponantur, operam navabunt.* In priore epistolæ parte, ubi de seriebus Newtonianis loquitur, se eas leviter percurrisse dicit, visurum si forte in eis inveniret Leibnitii seriem pro circulo hyperbolæ quadrandis.

N° LIV. Quod si in extractis Gregorianarum epistolarum eam inquisivisset, repperisset utique in epistolâ 15 Feb. 1671, suprâ memoratâ. Quippe extracta illa, in quibus ea habetur epistola, supersunt adhuc Collinii manu scripta.

N° XX,
XLVI.

Quamquam autem seriem illam, de quâ agitur, jam bis ab Oldenburgio Leibnitius accepisset, illam ipsam tamen, in epistolâ datâ 27 Aug. 1676, velut suam Oldenburgio remisit, quasi munus ἀντά. N° LII. ξίον pro methodo Newtoni; præ se ferens, se jam triennio antè, vel amplius, amicis suis Parisiensibus eam ostendisse; hoc est, biennio prius quàm eam accepisset in Oldenburgii epistolâ 15 Apr. 1675. Atqui illo tempore seriem illam suam esse nesciebat; uti constat ex ipsius responso 20 Maii 1675, suprâ citato. Fieri quidem potuit, ut Londini eam acceperit, ac amicis Parisiensibus ostenderit, trienniè prius quàm Oldenburgio eam remiserit: minimè tamen constat, se ejus demonstrationem tam maturè uactum esse. Hanc ubi primum reppererat, tunc demum in opusculo suo eam exhibuit, cumque amicis communicavit: idque ipse narrat contigisse anno 1675. Illud verò probandum & evincendum est, se prius eam penes se habuisse, quàm ab Oldenburgio eam accepisset. Quippe in responso suo ad Oldenburgium, nullam ex seriebus tunc missis suam esse sciebat; celabatque ab amicis Parisiensibus, se illam dum pluribus aliis ab Oldenburgio accepisse, ac vidisse se Gregorii epistolam, in quâ is Collinio eam miserat, in-eunte anno 1671.

In eadem epistolâ, 27 Aug. 1676, postquam descripserat quadraturam suam circuli hyperbolæque æquilateræ, hæc addit Leibnitius: *Vicissim, ex seriebus regressuum, pro hyperbolâ banc invenit.* N° LII. *Sit numerus aliquis, unitate minor, 1 - m; ejusque logarithmus hyperbolicus, k. Erit* $m = \frac{1}{1} - \frac{1^k}{1 \times 2} + \frac{1^k}{1 \times 2 \times 3} - \frac{1^k}{1 \times 2 \times 3 \times 4} + \&c.$ *Si numerus sit major unitate, ut 1 + n, tunc pro eo inveniendō mibi etiam prædiit regula, quæ in Newtoni epistolâ expressa est: scilicet erit* $n = \frac{1}{1} + \frac{1^k}{1 \times 2} + \frac{1^k}{1 \times 2 \times 3} + \frac{1^k}{1 \times 2 \times 3 \times 4} + \&c.$ *Quod regressum ex arcubus attinet; incideram ego directè in regulam, quæ ex dato arcu sinum complementi exhibet. Nempe sinus complementi* $= 1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4} - \&c.$ *Sed postea quoque deprehendi, ex eâ illam nobis communicatam, pro inveniendō sinu recto, qui est* $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} - \&c.$ *posse demonstrari.*

In his verbis Leibnitius sibi laudem vindicat co-inventionis quatuor harum serierum: quamvis methodus eas inveniendi, ipso expetente,

expetente, ad eum missa fuerit; quam tamen nondum intelligere, nec comprehendere poterat. In eadem utique epistolâ 27 Aug. 1676, orabat D. Newtonum, ut clarius eam explicaret.

Nº LII. Verba ipsius sunt: *Sed desideraverim, ut clarissimus Newtonus nonnulla quoque amplius explicet; ut originem theorematis, quod initio ponit: item modum quo quantitates p, q, r , in suis operationibus invenit: ac denique quomodo in methodo regressuum se gerat; ut cum ex logarithmo quærit numerum.* Neque enim explicat quomodo id ex methodo sua derivetur. Præ se tulit, invenisse se duas series pro numero cujus logarithmus sit datus; & tamen in ipsâ eâ epistolâ Newtonum rogat, ut methodum eas ipsas duas series invenendi sibi explicare velit.

Nº LXI. Ubi hanc ejus epistolam accepisset Newtonus, rescripsit se omnes illas quatuor series jam ei communicasse; quarum duæ priores una eademque series esset, literâ l pro logarithmo positâ cum suo signo $+$ vel $-$; tertia verò excessus esset radii supra sinum versam, pro quo jam antea series ad eum missa fuisset. His lectis, destitit Leibnitijs ab inventione hâc sibi vindicandâ. Præter hæc,

Nº LXIV. in eadem epistolâ 24 Oct. 1676, quod petierat Leibnitijs, methodos suas regressionis apertius explicavit. Leibnitijs tamen, epistolâ 21 Jun. 1677 datâ, ulteriorem adhuc petebat explicationem: paulo verò post, cum Newtoni epistolam repetitâ vix legisset, rescripsit 12 Jul. 1677, se jam, quod ignoraverat, intelligere; & ex chartis suis repositis animadvertere, se jam antea unam ex Newtoni methodis adhibuisse; in exemplo verò, quo fortè esset usus, cum nihil pulchri & elegantis proveniret, se, pro solitâ suâ impatientiâ, postea eam abjecisse. Plures itaque (si credere fas est) directas series, & proinde earum inveniendarum methodum habuit, priusquam invenisset methodum inversam, ejusque postea oblitus esset. Quod si chartas suas repositas diligentius pervolvisset, etiam hanc inversam methodum ibi repperisset. Sed, propriarum scilicet methodorum oblitus, Newtonianas desiderabat.

Nº L.

Cum Newtonus, in epistolâ datâ 13 Jun. 1676, methodum suam serierum enarrasset, hæc addidit: *Ex his videre, est quantum fines analysis per hujusmodi infinitas æquationes ampliantur: quippe quæ, earum beneficio, ad omnia penè dixerim problemata, si numeralia*

meralia Diophanti & similia excipias, sese extendit. Non tamen omnino universalis evadit, nisi per ultiores quasdam methodos eliciendi series infinitas. Sunt enim quedam problemata, in quibus non licet ad series infinitas per divisionem vel extractionem radicum, simplicium affectarumve, pervenire. Sed quomodo in istis casibus procedendum sit, jam non vacat dicere; ut neque alia quadam tradere, quæ circa reductionem infinitarum serierum in finitas, ubi rei natura tulerit, excogitavi. - Nam parcius scribo, quod hæc speculationes diu mihi fastidio esse ceperunt; adeo ut ab iisdem jam per quinque fere annos abstinuerim. His D. Leibnitijs, in epistolâ suâ Nº LIII 27 Aug. 1676 datâ, sic respondit: *Quod dicere videmini, plerasque difficultates (exceptis problematis Diophanteis) ad series infinitas reduci, id mihi non videtur. Sunt enim multa usque adeo mira & implexa, ut neque ab æquationibus pendeant, neque ex quadraturis. Qualia sunt, ex multis alijs, problemata methodi tangentium inversæ.* Et D. Newtonus in epistolâ suâ 24 Octob. 1676 rescripsit: Nº LXIV. *Ubi dixi omnia pene problemata solubilia existere; volui de iis præsertim intelligi, circa quæ Mathematici se hæcenus occuparunt; vel saltem in quibus racionia mathematica locum aliquem obtinere possunt. Nam alia sane adeo perplexis conditionibus implicita excogitare liceat, ut non satis comprehendere valeamus: & multo minùs tantarum computationum onus sustinere, quod ista requirerent. Attamen ne nimium dixisse videar, inversa de tangentibus problemata sunt in potestate, aliaque illis difficiliora. Ad quæ solvenda usus sum duplici methodo, unâ concinniori, alterâ generaliiori. Utramque visum est impræsentia literis transpositis consignare, ne propter alios idem obtinentes, institutum in aliquibus mutare coger. sacedæioeffh &c. id est: Una methodus consistit in extractione fluentis quantitatis, ex æquatione simul involvente fluxionem ejus: altera tantum in assumptione seriei pro quantitate quâlibet incognitâ, ex quâ cætera commodè derivari possunt; & in collatione terminorum homologorum æquationis resultantis, ad eruendos terminos assumptæ seriei.*

Ex duabus his Newtoni epistolis certò constat, jam tum, vel potius ante quinquennium, invenisse illum reductionem problematum ad æquationes fluxionales & series convergentes: & ex responso Leibniti ad harum epistolarum priorem, æquè certum est,

tum nondum hunc invenisse reductionem problematum, vel adæquationes differentiales, vel ad series convergentes.

Nº
XXXVIII.

Idque amplius ex eis manifestum est, quæ de hac re scripsit Leibnitiuss anno 1691, in Actis Eruditorum: *Jam anno 1675, inquit, compositum habebam opusculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum; sed quod, materiâ sub manibus crescente, limare ad editionem non vacavit, postquam aliæ occupationes supervenire; præsertim cum nunc prolixius exponere, vulgari more, quæ analysis nostra paucis exhibet, non satis operæ pretium videatur.* Hanc Quadraturam, vulgari more compositam, proferre cœpit Parisiis anno 1675. Anno proximo demonstrationem ejus expoliabat, Oldenburgio mittendam, ceu methodi Newtonianæ ἀνάλυσις; ut narrat in epistolâ 12 Maii 1676: & proinde, in epistolâ 27 Aug. 1676, eam misit contextam & edolatam more vulgari. Hieme insequente, in Germaniam reversus per Angliam & Hollandiam, ut negotia publica capefferet, non vacavit amplius ad eam prælo parandam, nec operæ pretium existimavit, ea more vulgari prolixius explicare, quæ analysis ejus paucis exhibet. Hanc ergo novam analysin excogitavit, jam in Germaniam reversus; & proinde non ante annum 1677.

Nº XLIV.

Idque amplius adhuc constat ex consideratione sequenti. Barrovius methodum suam tangentium anno 1670 in lucem edidit. Inde Gregorius methodum tangentium hausit absque computatione, uti ad Collinium scripsit 5 Sept. 1670. Newtonus autem suam tangentium cum Collinio communicavit anno 1672, in epistolâ 10 Decemb. datâ, atque hæc ibi addidit. *Hoc est unum particulare, vel Corollarium potius, methodi generalis; quæ extendit se, citra molestum ullum calculum, non modo ad ducendum tangentes ad quasvis Curvas, sive geometricas, sive mechanicas, vel quomodocunque rectas lineas, aliasve Curvas respicientes; verum etiam ad resolvendum alia abstrusiora problematum genera de curvitatibus, areis, longitudinibus, centris gravitatis Curvarum, &c. Neque, quemadmodum Huddenii methodus de maximis & minimis, ad solas restringitur æquationes illas, quæ quantitibus surdis sunt immunes. Hanc methodum intertexui alteri isti, quâ æquationum exegesis insituo, reducendo eas ad series infinitas.* D. autem Slusius suam tangentium methodum ad Oldenburgium misit 17 Jan.

1673, eaque paulo post in Transactionibus est publicata. Composita verò est eadem prorsus esse cum illâ Newtoni. Fundata erat super tribus lemmatibus; quorum primum erat, *Differentia duarum dignitatum ejusdem gradûs applicata ad differentiam laterum dat partes singulares gradûs inferioris ex binomio laterum, ut* $\frac{y^2 - x^2}{y - x} = yy + yx + xx$, *id est, secundum notationem Leibnitii, $\frac{dy^2}{dy} = 3yy$.*

Newtonianæ epistolæ, 10 Decemb. 1672, exemplar ad Leibnitiũ ab Oldenburgio missum est, inter chartas Jacobi Gregorii, unâ cum aliâ Newtoni epistolâ 13 Jun. 1676 datâ. In his duabus cum memoraret Newtonus, se generalem admodum analysin habere, partim consistentem ex methodo serierum convergentium, partim ex aliâ methodo, quâ applicabat eas series ad solutionem omnium ferè problematum, exceptis forte numeralibus quibusdam quales illæ sunt Diophanti, eruebatque tangentes, areas, longitudines, contenta solida, centra gravitatis, curvitatesque Curvarum ac curvilinearum figurarum, seu geometricarum, sive mechanicarum, minimè hærendo ad furdas; methodumque illam tangentium Slusianam non nisi ramum, vel corollarium, esse alterius hujus methodi: his Leibnitiuss visis, dum domum per Hollandiam reverteretur, tum demum meditabatur promotionem methodi Slusianæ. Quippe in epistolâ ad Oldenburgium Amstelodami datâ 18 Novemb. 1676, sic scripsit: *Methodus tangentium, à Slusio publicata, nondum rei fastigium tenet. Potest aliquid amplius præstari in eo genere, quod maximi foret usus ad omnis generis problemata, etiam ad meam (sine extractionibus) æquationum ad series reductionem. Nimirum posset brevis quædam calculi circa tangentes tabula, eousque continuanda donec progressio tabulæ apparet; ut eam scilicet quisque, quousque libuerit, sine calculo continuare possit.* Hæc verò erat illa promotio Slusianæ methodi in methodum generalem, quam tum in animo versabat Leibnitiuss: exque illis ejus. verbis, *potest aliquid amplius præstari in eo genere, quod maximi foret usus ad omnis generis problemata*, unica res hæc fuisse videtur, quâ ille methodum eam ad omnis generis problemata vellet extendere. Promotio verò per calculum differentialem nondum ei in mentem venerat; ea quippe referenda erit ad annum sequentem.

Nº XLV.

Nº XVI.

Nº XXVI.

N° LVII.

In proximis literis, 24 Octob. 1676, mentionem analyseos suæ fecit Newtonus, communicatæ per Barrovium cum Collinio anno 1669; alteriusque item tractatus, anno 1671 scripti, de seriebus convergentibus, deque alterâ illâ methodo, quâ tangentes ducebantur more Slusii, maximæque ac minimæ determinarentur, & quadratura Curvarum expeditior fieret, idque non hæsitando ad radicales; quâque invenirentur series, quæ certis casibus finirentur, & quadraturam Curvarum darent in æquationibus finitis, ubi fieri posset. Fundamentum autem harum operationum conclusit in hanc sententiam, ænigmaticè, ut suprâ, expressam; *Data æquatione fluentes quotcunque quantitates involvente, fluxiones invenire; & vice versâ.* Quibus extra omnem dubitationem ponitur, se jam antea fluxionum methodum excogitasse. Quod si reliqua in epistolâ illâ animadvertantur; constabit utique, se methodum illam jam tum ad magnam perfectionem provexisse, & fecisse admodum generalem: cum illæ in libro suo Quadraturarum propositiones, methodique serierum convergentium, lineamque curvam ducendi per quémvis datorum punctorum numerum, jam tum sibi innotuerunt. Quippe, cum fluxionum methodus haud procedit in æquationibus finitis, æquationes in series convergentes reducit per theorema binomiale, perque fluentium extractionem ex æquationibus, fluxiones earum involventibus vel non involventibus. Cumque æquationes finitæ defuerint, series convergentes ex problematis conditionibus deducit; assumendo terminos serierum gradatim, & per conditiones illas determinando. Cumque porro fluentes à fluxionibus sint derivandæ, & fluxionum lex defuerit; legem eam invenit quamproximè, parabolicam lineam per quemlibet datorum punctorum numerum ducendo. Atque his progressionibus, vel illo tempore fluxionum suam methodum multo magis universalem fecerat Newtonus, quàm vel hodie est methodus Leibnitii differentialis.

N° LXVI.

Hæc Newtoni epistola, data 24 Octob. 1676, in fine mensis illius, vel initio sequentis, visa est Leibnitio Londini; ejusque exemplar Hanoveriæ ei obtigit, initio veris insequentis: atque ipse paulo post Leibnitius, epistolâ datâ 21 Jun. 1677, rescripsit: *Clarissimi Slusii methodum tangentium nondum esse absolutam, celeberrimo Newtono assentior. Et jam à multo tempore rem tangentium generalius*

generalius tractavi, scilicet per differentias ordinarum—Hinc nominando in posterum; dy differentiam duarum proximarum y, &c. Hic demum primò cœpit Leibnitius differentialem suam methodum proferre: neque vel minimum argumentum est, prius eam se scivisse, quàm postremas Newtoni literas accepisset. Dicit quidem, *jam à multo tempore rem tangentium generalius se tractavisse, scilicet per differentias ordinarum.* Atqui in aliis literis eodem modo jam affirmaverat, se plures convergentes series, tam directas quàm inversas, invenisse; priusquam ullam inveniendi eas methodum haberet: oblitumque jam fuisse inversæ methodi serierum; priusquam utilitatem ejus perciperet. Nemo in causâ propriâ sibi testis est. Iniquus admodum fuerit iudex, omniumque gentium jura conculcaverit, qui quemquam in suâ causâ pro legitimo teste admiserit. Illud ergo est probandum ac ostendendum, jam antea methodum hanc Leibnitium invenisse, quàm literas illas Newtoni accepisset. Quod si hoc nullo argumento confirmatum fuerit; de primo methodi inventore nulla superest controversia.

Marchio Hospitalius, vir candidissimus, in præfatione libri sui *De Analyse quantitatum infinite parvarum*, A. D. 1696 editâ, narrat; ut, paulo post tangentium methodum à Cartesio publicatam, Fermatius quoque methodum invenerit, quam ipse tandem Cartesius suâ in plerisque simpliciore esse confessus est. 'Non dum tamen, inquit Hospitalius, tam simplex erat, quàm postea à Barrovio reddita est, naturam polygonorum propius considerando; quæ sponte suâ menti objicit parvulum triangulum, compositum ex particulâ Curvæ, inter duas ordinatas sibi infinite propinquas jacentis, & ex differentiâ duarum istarum ordinatarum, duarumque itidem correspondentium abscissarum. Atque hoc triangulum illi simile est, quod ex tangente et ordinatâ & subtangente fieri debet: adeo ut per unam simplicem analogiam omnis jam calculatio evitetur, quæ & in Cartesiana, & in hac ipsâ prius methodo, necessaria erat. Quo tamen vel hæc, vel Cartesiana, revocari ad usum posset, necessariò tollendæ erant fractiones & radicales. Ob hujus itaque calculi imperfectionem, introductus est ille alter celeberrimi Leibnitii; qui insignis geometra inde est exorsus, ubi Barrovius aliique desierant. Porro

Porro hic ejus calculus in regiones hactenus ignotus aditum fecit; atque ibi tot & tanta patefecit, quæ vel doctissimos totius Europæ Mathematicos in admirationem conjecerunt, &c.

Hactenus Hospitalius. Non viderat nimirum Newtoni analyfin, neque epistolas ejus 10 Dec. 1672, 13 Jun. 1676, & 24 Octob. 1676 datas: quarum nulla ante annum 1699 typis publicata est. Nescius itaque Newtonum hæc omnia effecisse, atque indicasse Leibnitio, Leibnitium ipsum arbitratus est inde incepisse, ubi desierat Barrovius; Leibnitium docuisse, quo pacto Barrovii methodus adhiberetur non hærendo ad fractiones & furdas, eaque remirificè eam ampliâsse & promovisse. Similiterque Jacobus Bernoullius in Actis Eruditorum Jan. 1691, p. 14, sic memorat: *Qui calculum Barroviaum, quem in lectionibus suis geometricis adumbravit auctor, cujusque specimina sunt tota illa propositionum inibi contentarum farrago, intellexerit; [calculum] alterum, à domino Leibnitio inventum, ignorare vix poterit; utpote qui in priori illo fundatus est, & nisi fortè in differentialium notatione, & operationis aliquo compendio, ab eo non differt.*

Jam verò, in methodo suâ tangentium, Barrovius ducit duas ordinatas indefinitè sibi invicem propinquas, literamque *a* ponit pro ordinatarum differentiâ, proque abscissarum differentiâ literam *e*; & in ducendis tangentibus has tres regulas statuit: 1. *Inter computandum, inquit, omnes abjicio terminos, in quibus ipsarum a vel e potestas habeatur, vel in quibus ipsæ ducuntur in se. Et enim ipsi termini nihil valebunt.* 2. *Post æquationem constitutam, omnes abjicio terminos, literis constantes quantitates notas seu determinatas significantibus, aut in quibus non habentur a vel e. Et enim illi termini semper ad unam æquationis partem adducti, nihilum adæquabunt.* 3. *Pro a ordinatam, & pro e subtangentem substituo. Hinc demum subtangentis quantitas dignoscitur.*

Nº LXVI.

Hactenus Barrovius. Leibnitius autem in epistolâ 21 Jun. 1677 supra citatâ, in quâ primò differentialem suam methodum cœpit proponere, Barrovia nam hanc tangentium methodum exactè secutus est; præterquam quòd literas, *a* & *e*, Barrovia nas mutaverit in *dx* & *dy*. Quippe in exemplo, quod ibi exhibet, duas ducit parallelas lineas; atque omnes terminos sub inferiore lineâ ponit, in quibus *dx* & *dy* (divisim vel junctim) sunt plus annius dimensionis;

sionis; omnes verò terminos, in quibus *dx* & *dy* absunt; super lineam superiorem statuit; &, ob rationes à Barrovio datas, omnes hos terminos facit evanescere. Jam autem per terminos in quibus *dx* & *dy* unius tantum dimensionis sunt, quosque inter binas illas lineas ponit, proportionem subtangentis ad ordinatam determinat. Rectè itaque animadvertit Marchio Hospitalius, Leibnitium inde incipere, ubi Barrovius desierat; quippe utriusque methodus tangentium prorsus est eadem.

Illud tamen Leibnitius de hac methodo superannotat; conclusionem nempe hujus calculi cum Slusii regulâ coincidere; illamque regulam cuivis, qui hanc methodum intelligat, in promptu occurrere. Acutè fanè: quippe in epistolis suis Newtonus indicaverat, Slusianam regulam generalis suæ methodi Corollarium tantum esse.

Cumque in epistolis Newtonus dixisset, in ducendis tangentibus, maximisque & minimis determinandis, methodum suam procedere, non hæsitando ad furdas; Leibnitius itidem annotat, sic promoveri posse tangentium suam methodum, ut ad furdas & fractiones non hæreamus; & deinde addit: *Arbitror, quæ celare Nº LXVI. voluit Newtonus, de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc eodem fundamento quadraturas quoque reddi faciliores, me in hac sententiâ confirmat; nimirum semper figuræ illæ sunt quadrabiles, quæ sunt ad æquationem differentialem.* Ex quibus ejus verbis, cum præcedente calculatione comparatis, non dubium est, quin tum satis sciverit Leibnitius, Newtono ad manum fuisse methodum hæc omnia efficientem; apparetque eum tentavisse, si fortè differentialis tangentium methodus Barrovia na ad eadem efficienda promoveri posset.

Differentialis hujus methodi elementa publicavit Leibnitius in Actis Eruditorum Nov. 1684; exemplisque ducendi tangentes, maximasque & minimas determinandi, eam illustravit; quibus addit: *Et hæc quidem initia sunt geometriæ cujusdam multo sublimioris, ad difficillima ac pulcherrima quæque etiam mixtæ matheſeos problemata pertingentis; quæ sine calculo differentiali, AUT SIMILI, non temerè quisquam pari facilitate tractabit.* Ubi, cum dicit AUT SIMILI, sine dubio ad Newtoni methodum respexit: totaque ista periodus

periodus nihil ampliùs in se habet, quàm quod Newtonus, in literis 1672 & 1676, de suâ generali methodo affirmaverat.

Et in Actis Eruditorum 1686, p. 279, *Malo autem, inquit Leibnitiùs, dx & similia adhibere, quàm literas pro illis, quia illud dx est modificatio quædam ipsius x , &c.* Sciebat scilicet in hac methodo literas, more Barrovii, satis commodè posse adhiberi; malebat tamen novis symbolis uti, dx & dy ; etsi nihil per hæc symbola fieri possit, quod non brevius commodiusque per singulas literas fiat.

Anno sequente in lucem edita sunt Newtoni Principia Philosophiæ; refertus liber ejusmodi problematibus, qualia Leibnitiùs difficillima appellaverat, & pulcherrima. etiam mixtæ matheos problemata; quæ sine calculo differentiali, aut simili, non temerè ququam pari facilitate tractabit. De hoc libro sic locutus est Marchio Hospitaliùs, quasi totus ferè per hunc calculum compositus esset. Et ipse adeo Leibnitiùs, epistolâ ad Newtonum datâ Hanoveriæ 7 Martii 1693, ipsiusque manu scriptâ, quæ adhuc superest, & Regiæ Societati nuper est exhibita, eandem rem agnoscebat his verbis: *Mirificè ampliaveras geometriam tuis seriebus; sed edito Principiorum opere, ostendisti patere tibi, quæ analysi receptæ non subsunt.* Conatus sum ego quoque, notis commodis adhibitis, quæ differentias & summas exhibeant, geometriam illam, quam transcendente appello, analysi quodammodo subicere; nec res malè processit. Atque iterum in responso ad D. Fatium, quod habetur in Actis Eruditorum Maii 1700, p. 203, versu 21, id fassus est Leibnitiùs.

In secundi libri Principiorum lemmate secundo, elementa hujus calculi syntheticè demonstrata sunt; & in fine lemmatis est scholium, his verbis: *In literis, quæ mihi cum Geometrà peritissimo G. G. Leibnitiùs annis abhinc decem intercedebant, cum significarem me compotem esse methodi determinandi maximas & minimas, duccendi tangentis & similia peragendi, quæ in terminis furdis, æquè ac in rationalibus, procederet; & literis transpositis banc sententiam involventibus [Datâ æquatione quòtcunque quantitates fluentes involvente, fluxiones invenire, & vice versâ] eandem celarem: rescripsit vir clarissimus [anno proximo] se quoque in ejusmodi methodum incidisse; & methodum suam communicavit, à meâ vix abluentem*

dentem, præterquam in verborum & notarum formulis. Utriusque fundamentum continetur in hoc lemmate. In illis epistolis, & in n^o xxvi. aliâ 10 Decemb. 1672 datâ (cujus exemplar post annos quatuor ab Oldenburgio ad Leibnitiùm mittebatur, ut suprà diximus) adeo apertè explicaverat suam methodum Newtonus; ut non difficile fuerit Leibnitiùs, subsidio methodi tangentium Barrovianæ, ex illis epistolis eam exsculpere. Certum tamen est, ex argumentis suprà allatis, non priùs eum scivisse eam, quàm epistolas illas legisset.

Duarum Newtoni epistolarum, 13 Jun. & 24 Octob. 1676, exemplar ab Oldenburgio acceperat Wallisiùs, & ex eis plura publicaverat in Algebrâ suâ, Anglicè editâ 1683, Latinè autem 1693; Operum Vol. II. p. 386. pauloque post ex Hollandiâ admonitus est, ut epistolas illas integras publicaret; quia notiones de fluxionibus Newtonianæ cum plausu ibi per hominum ora ferrentur, sub nomine methodi differentialis Leibnitiùs. Quamobrem in præfatione primi suorum operum Tomi, A. D. 1695 editi, ejus rei mentionem iniecit. Et in epistolâ ad Leibnitiùm, datâ 1 Dec. 1696, quæ in tertio tomò extat, hæc de eâ re habet: *Cùm præfationis præfigendæ postremum folium erat sub prælo, ejusque typos jam posuerant typothetæ; me monuit amicus quidam, harum rerum gnarus, qui peregrè fuerat, tum talem methodum in Belgio prædicari, tum illam cum Newtoni methodo fluxionum quasi coincidere. Quod fecit ut, translatis typis jam positis, monitum interseruerim.* Quin & in epistolâ ad Newtonum, datâ 10 April. 1695 & Regiæ Societati nuper exhibitâ, sic de eâ re verba facit: *Utinam typis ederes prolixas illas duas epistolas Junii & Augusti (Octobrem dicere debuit) 1676. Ex Hollandiâ certior factus sum, amicos ibi tuos hoc postulare; quia notiones tuæ de fluxionibus Leibnitiùs ibi ascribuntur, sub nomine calculi differentialis. Hoc ex Hollandiâ accepi, cùm totus hic tomus, præter partem præfationis, jam prælo subiectus esset; ita ut nihil aliud inferere hîc potuerim, dum cessarent operæ, præter brevem illam, quam ibi reperies, narrationem. Non tam æquus es vel tuo vel gentis tuæ bonori, quàm oportebat: cùm res quantivis pretii tam diu in scriniis celas, donec alii honorem tibi debitum præripiant. Conatus sum in eo negotio debitum tibi reddere; doleoque me non binas illas epistolas integras atque autologè edidisse.*

Porro illa brevis mentio, quam Wallisius præfationi illi inferuit, his verbis habetur: *In secundo volumine inter alia habetur Newtoni methodus de Fluxionibus, ut ille loquitur, consimilis naturæ cum Leibnitii, ut hic loquitur, calculo differentiali; quod, qui utramque methodum contulerit, satis advertat; utut sub loquendi formulis diversis: quam ego descripsi (Algebræ, Cap. xci. &c. præsertim, Cap. xcv.) ex binis Newtoni literis, aut earum alteris, Junii 13, & Octob. 24, 1676, ad Oldenburgium datis, cum Leibnitio communicandis; iisdem ferè verbis, saltem leviter mutatis, quæ in illis literis habentur, ubi METHODUM HANC LEIBNITIO EXPONIT, tum antedecem annos nedum plures [id est anno 1666 vel 1665] ab ipso excogitatam. Quod moneo, ne quis causetur, de hoc calculo differentiali nihil à nobis dictum esse.*

His ad hunc modum actis, anni sequentis mensè Junio, editores Actorum Lipsiensium, vel potius, ut ex stylo colligitur, ipse Leibnitius, cum de duobus prioribus Wallisii tomis narrationem contexunt, hujus in præfatione clausulæ mentionem fecerunt; quæstique sunt, non quòd dixerit Newtonum, in duabus illis epistolis, explicuisse Leibnitio fluxionum methodum, decennio antè vel amplius à se inventam; sed quòd, de calculo differentiali verba faciens, eamque ut ait ob rationem, *ne quis causetur de calculo differentiali nihil ab ipso dictum fuisse*, non monuerit lectorem, jam tum Leibnitium calculum illum penes se habuisse, cum mutæ illæ inter ipsum & Newtonum literæ, Oldenburgii operâ, hinc inde scriberentur. Et in pluribus post illa epistolis, inter Leibnitium & Wallisium de eâ re conscriptis, non negabat Leibnitius, Newtonum, toto ante eas literas datas decennio, dictam methodum invenisse; id quod affirmaverat ibi Wallisius: non præ se ferebat, tam maturè se suam methodum excogitasse; nullo argumento probabat, se ante annum 1677 in eam incidisse; neque id ipsum probabat, nisi ex Newtoni concessio; non affirmabat ipse, se maturius habuisse; laudabat Newtonum, quòd in hac re tam candidè egerit; concedebat, utramque methodum eodem in summa recidere; seque idcirco solitum, communi Analyseos infinitesimæ nomine utramque indigitare: adjiciebat, sicuti Vietæ Cartesiique methodi, communi Analyseos speciosæ nomine ferebantur, licet in aliquibus differrent; ita fortè suam Newtonique methodos

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ensium editores (hoc est, ipse Leibnitius) cum de tractatu hoc N° LXXIX. agerent, affirmabant Leibnitium fuisse primum ejus methodi inventorem, & Newtonum pro differentiis fluxiones substituiffe. Atque hæc affirmatio ortum dedit præfenti controversiæ.

N° LXXX. Quippe D. Keillius, in epistolâ in Transactionibus Philosophicis editâ, retorfit in eos hoc telum : *Fluxionum, inquit, Arithmetica sine omni dubio primus invenit D. Newtonus ; ut cuilibet ejus epistolas, à Walliso editas, legenti facili constabit. Eadem tamen Arithmetica postea, mutatis nomine & notationis modo, à D. Leibnitio in Actis Eruditorum edita est.*

Newtonus, priusquam vidisset id, quod in actis Lipsiensibus publicatum fuerat, ægrè tulit à D. Keillio hoc dictum esse ; ne fortè inde lis aliqua nasceretur. Leibnitius quoque, hoc acerbius interpretans quàm vel à Keillio cogitatum fuerat, in literis ad D. N° LXXX. Sloane datis 4 Martii 1711, de hoc ut calumniâ questus est ; petiitque, ut Regia Societatis injungeret Keillio, palinodiam ut publicè caneret. Keillius verò id quod scriptum erat probaturum se ac defensurum profitetur ; licentiâ à Newtono, cui quod in Actis Lipsiensibus dictum est ostendebatur, impetratâ. Leibnitius autem, in alterâ ad D. Sloane epistolâ 29 Decemb. 1711 datâ, neglectâ accusationis suæ probatione, candorem modò suum prædicare, de quo vel dubitare incivile foret ; non modum ostendere, quo methodum invenisset ; in Actis Lipsiensibus suum cuique datum esse ; se inventionem novem annis (septem credo dicere debuit) penes se celavisse, ne quisquam (Newtonum intelligit) eam sibi præripuisse gloriatur ; Keillium esse hominem juvenem, rerum antea actarum ignarum ; dixisse illud, Newtono nolente ; rixosum porro hominem esse, cui silentium imponi debeat ; se cupere, ut Newtonus ipse sententiam de hâc re suam pronuntiaret. Atqui satis noverat, nihil amplius Keillium dixisse, quàm quod tredecim ante annis, nullo tum contra eunte, dixerat Wallisius : noverat Newtonum sententiam de hâc re tulisse, in introductione ad librum Quadraturarum, prius in lucem editum, quàm hæc lis moveretur. Wallisius verò jam ad plures abierat : qui restabant in Angliâ Mathematici, pro noviciis habentur : de cujusvis candore Leibnitius jure suo dubitare volet ; Newtonus denique, nisi vel

vel dissimulaverit rem, vel abnegaverit, in rixas & molestias trahendus est.

Regia itaque Societas, cujus auctoritati non minus Leibnitius, quàm Keillius (uterque scilicet in ea Socii) parere debebant, bis à Leibnitio huc provocata, nefasque esse existimans, vel damnare vel notare Keillium, re nondum examinatâ ; sciensque nec Newtonum neque Leibnitium, qui in vivis soli vel scirent quid, vel meminissent, quod in his rebus ante annos quadraginta actum sit, in hâc Keillii causâ testes esse posse ; negotium id dederunt numero ex Societate confessui, ut excuterent veteres in Archivis suis Epistolas & Chartas, & quid in eis de hoc negotio reperissent, Sô N° LXXXVI. cietati exponerent : quam expositionem ut primum Societas acceperat, & ipsam, & Epistolas, Chartasque ipsas, in publicum edijussit. Ceterum ex illis id Confessui compertum visum est, Newtonum anno 1669, vel antea, methodum illam penes se habuisse ; Leibnitium verò non ante annum 1677.

Ut methodi differentialis primum se auctorem vendicaret Leibnitius, insimulavit Newtonum, literâ o , more vulgari, pro dato incremento rs x primò fuisse usum ; qui mos differentialis methodi utilitates tollit : post edita verò Principia mutavisse o in x , substituendo x pro dx . Hoc verò nunquam quis probaverit ; Newtonum unquam o in x mutavisse, vel usurpasse x pro dx , vel omisisse uti literâ o . Newtonus in Analyfi, anno 1669 vel antea scriptâ, & in libro de Quadraturis, & in Principiis Philosophiæ usus est literâ o ; atque adhuc utitur, eodem planè quo prius sensu. In libro de Quadraturis usus est literâ o unâ cum symbolo x ; ideoque non posuit unum loco alterius. Symbola ista, o & x , pro rebus diversi generis posita sunt. Prius est momentum ; alterum fluxio est, sive velocitas ; ut suprâ est explicatum. Cum litera x pro quantitate uniformiter fluente ponitur, symbolum x est unitas ; & litera o (seu 1^o) momentum ; atque xo & dx idem ambo momentum significant. Literæ punctatæ numquam indicant momenta ; nisi cum multiplicantur per momentum o , vel expressum vel subintellectum, quo infinite parvæ evadant ; & tum rectangula pro momentis ponuntur.

Newtonus non in formis symbolorum suam methodum constituit, neque se alligat ad ullam unam speciem symbolorum pro fluentibus

N° XII.

N° VIII.

fluentibus & fluxionibus. Ubi areas Curvarum pro fluentibus ponit, sæpe ponit ordinatas pro fluxionibus, & fluxiones denotat per symbola ordinarum; ut in Analyfi suâ fecit. Ubi lineas pro fluentibus ponit, quævis symbola ponit pro velocitatibus punctorum lineas describentium, hoc est, pro fluxionibus primis; & quævis alia symbola, pro incremento earum velocitatum, hoc est, pro fluxionibus secundis; ut sæpe fit in Principiis Philosophiæ. Ubi autem literas x, y, z , pro fluentibus ponit; earum fluxiones denotat vel per alias literas, ut p, q, r ; vel per eandem literas aliâ formâ positas, ut x, y, z ; vel $\dot{x}, \dot{y}, \dot{z}$, punctatas; vel per quavis lineas, ut DE, FG, HI , consideratas tamquam earum exponentes. Atque hoc quidem manifestum est ex libro ejus de Quadraturis: ubi, in primâ propositione, fluxiones indicat per literas punctatas; in ultimâ propositione, per ordinatas Curvarum; & in introductione, per alia symbola, dum methodum explicat, illustratque per exempla. Leibnitius, in suâ methodo, nulla fluxionum symbola habet; & idcirco Newtoniana fluxionum symbola sunt in eo genere prima. Leibnitius symbolis illis momentorum, five differentiarum, dx, dy, dz , primò uti cœpit anno 1677: Newtonus momenta denotabat per rectangula sub fluxionibus & momento o , cùm Analyfin suam scriberet, anno 1669 vel antea. Leibnitius symbolis $\dot{x}, \dot{y}, \dot{z}$, pro summis ordinarum usus est, jam inde ab anno 1686: Newtonus, in Analyfi suâ, eandem rem denotavit, inscribendo ordinatam in quadrato vel rectangulo, ad hunc modum; $\frac{aa}{64x}$. Omnia Newtoni symbola sunt in suo quæque genere prima.

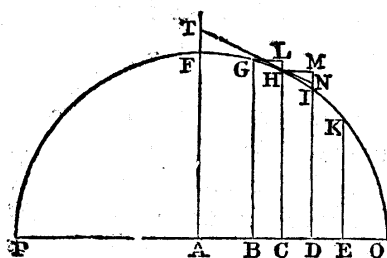
Quandoquidem autem infimulatum est, usum literæ o vulgarem esse, ac methodi differentialis utilitates tollere; è contrario, fluxionum methodus, prout à Newtono usurpata est, omnes differentialis methodi utilitates habet, & præterea alias. Elegantior est; quippe in ejus calculo una tantum infinitè parva quantitas est per symbolum denotata, idque symbolum est o . Nullas quantitatum infinitè parvarum ideas habemus: & idcirco in suam methodum fluxiones introduxit Newtonus, ut, quantum fieri posset, per finitas quantitates procederet. Naturalis magis est magisque geometrica; fundata scilicet super primis quantitatum nascentium

centium rationibus, quæ existentiam in geometria habent: cùm indivisibilia contrà, super quibus fundata est differentialis methodus, nullam existentiam habeant, nec in geometriâ neque in naturâ. Sunt quidem rationes primæ quantitatum nascentium; at non sunt quantitates primæ nascentes. Natura quantitates generat per continuum fluxum, five incrementum: talemque arearum & Solidorum generationem admiserunt veteres Geometriæ; cùm lineam unam in aliam ducerent, per motum localem, ad generandam aream; atque aream in lineam per motum localem ducerent, ad generandum Solidum: at computatio indivisibilium, ut inde componatur area vel Solidum, nunquam in hunc usque diem in geometriâ locum habuit. Porro Newtoniana methodus utilior quoque est illâ alterâ, atque certior; quippe adaptata & ad promptè inveniendam propositionem per tales approximationes, quales in conclusione nullum errorem creent, & ad eam exactè demonstrandam: Leibnitiana verò methodus ad inveniendam tantum propositionem, non ad demonstrandam accommodata est. Cùm operatio non succedat in æquationibus finitis, confugere solet Newtonus ad series convergentes; unde methodus ejus fit incomparabiliter magis universalis, quàm illa Leibnitii, quæ intra finitas æquationes terminatur: siquidem ille nullam partem habet in infinitarum serierum methodo. Annis post aliquot quàm serierum methodus inventa est, Leibnitius propositionem invenit pro transmutandis curvilinearibus figuris, in alias æqualium arearum curvilineares, ut inde per series convergentes quadrentur; at methodi figuras illas alias, per tales series, quadrandi non erant Leibnitii. Ope novæ illius Analyseos, majorem illarum propositionum partem, quæ in Principiis Philosophiæ habentur, invenit Newtonus. At cùm antiqui Geometriæ, quo certiora omnia fierent, nihil in geometriam admiserint, priusquam synthetice demonstratum esset; idcirco propositiones suas synthetice demonstravit Newtonus, ut cœlorum systema super certâ geometriâ constitueretur. Atque ea causâ est, cur homines harum rerum imperiti analyfin latentem, cujus ope propositiones illæ inventæ sunt, difficulter admodum perspiciant.

Infimulatum est, Newtonum, in scholio sub finem libri de Quadraturis, posuisse tertium, quartum, quintumque terminos seriei convergentis

convergentis respectivè æquales secundæ, tertiæ, quartæque differentis primi termini; & proinde methodum secundæ, tertiæ, quartæque differentiarum tum non intellexisse. Atqui in primâ libri ejus propositione, anno 1693 à Wallisio editâ, modum ostendit inveniendi primam, secundam, tertiam, sequentesque fluxiones in infinitum: ac proinde cum librum eum scriberet, ante annum nempe 1676, omnium omnino fluxionum inveniendarum methodum intellexit; & consequenter, omnium differentiarum. Quod si eam non intellexit, cum anno 1704 scholium illud in fine libri subjunxerit: necesse est hoc eo contigisse, quod per istorum annorum intervallum de memoriâ fortè ei exciderat. Hoc solum igitur disquirendum est, oblitus ne fuit methodi secundarum tertiarumque differentiarum ante annum 1704.

Principiorum Philosophiæ libri secundi propositione decimâ, cum exponeret utilitates aliquot terminorum convergentis seriei ad solvenda problemata, hoc docet Newtonus: Si primus nempe seriei terminus repræsentet ordinatam, CH, cujuscumque curvæ lineæ, PHQ; & CHDM sit parallelogrammum infinitè exile, cujus



latus, DM, secet Curvam in I, & tangentem ejus, HN, in N; tum secundus seriei terminus repræsentabit lineam MN, & tertius terminus lineam NI. Atqui linea NI dimidium tantum est secundæ

differentiæ ordinatæ: & proinde, cum Principia sua Newtonus scriberet, tertium terminum seriei æqualem posuit dimidio secundæ differentiæ termini primi: & consequenter, non oblitus tum erat methodi differentiarum secundarum.

Dum in eo opere versaretur, sæpissimè ei considerandum erat incrementum vel decrementum velocitatum, quibuscum quantitates generantur; inque eâ re rectè argumentatur. Atqui incrementum illud, vel decrementum, est ipsa secunda fluxio quantitatibus: non ergo oblitus tum erat methodi fluxionum secundarum.

Anno 1692 Newtonus, à Wallisio rogatus, misit ei propositionem primam libri de Quadraturis, cum exemplis ejus in primis secundis tertiisque fluxionibus; id quod cuivis videre est in Wallisii operum tomo secundo (anno 1693 edito) pag. 391, 392,

393 & 396. Ideoque ne tum quidem oblitus erat methodi secundarum fluxionum.

Nec sane verisimile est, se anno 1704, cum dictum scholium adderet fine libri de Quadraturis, oblitum esse non solum primæ ipsius illius libri propositionis, sed & ultimæ quoque, ad quam scholium istud subtextum erat. Si vocula *ut*, quæ in scholio illo inter verba *erit & ejus casu* aliquo excidisse potuit, ibi reponatur; tum scholium istud & duabus illis propositionibus, & ceteris Newtoni scriptis, congruet: & frustra omnino erunt, qui oblivionem hinc cavillantur.

Atque hætenus de naturâ atque historiâ harum methodorum egimus: porro haud abs re fuerit, de toto hoc negotio observationes pauculas subjungere.

In Commercio hoc Epistolico, tres memorantur Leibnitii tractatus N° LXXII. tatus; scripti nempe omnes postquam exemplar Principiorum Newtoni Hanoveriam ei missum fuerat; postquam viderat quoque ejusdem libri recensionem, in Actis Eruditorum Jan. & Feb. 1689. In his verò Leibnitii tractatibus, primariæ Newtoniani libri propositiones novo modo recomponuntur, Leibnitioque arrogatur; quasi prius eas ipse invenerat, quàm Newtoni liber ederet. Quis testem in suâ ipsius causâ patienter ferat? Vel fidem faciat Leibnitius, se, ante Newtoni librum editum, eas excogitasse; vel de eis sibi vindicandis pudorem habeat.

In tractatuum illorum postremo, vicesima propositio (quæ omnium Newtonianarum primaria est) Corollarium fit propositionis decimæ nonæ. Atqui decima illa nona demonstrationem sibi annexam habet *παράλογον* & falsam. Aut evincat itaque Leibnitius, demonstrationem illam non falsam esse; aut fateatur se 19 & 20 propositiones non ejus demonstrationis ope repperisse; sed, quo Newtoni eximiam illam propositionem pro suâ vendicaret, demonstrationem ejus extundere frustra tentavisse. Quippe in xx Propositione præ se fert, nescisse se, quâ eam viâ Newtonus invenerat; ut fidem scilicet lectori faceret, se, sine illius ope, eandem repperisse.

Ex erroribus in xv^a & xix^a Leibnitii propositione commissis, ostenderat Keillius, Leibnitium, cum tres illos tractatus scriberet, operandi vias in secundis differentis non optime calluisse. Id

quod amplius adhuc constat, ex illius Tractatus tertii Propositionibus x, xi, & xii. Has enim constituit ceu fundamentum infinitesimalis suae Analyseos, in considerandis viribus centrifugis; & decimam quidem proponit in relatione ad centrum curvatis orbis; in undecima tamen & duodecima eam adhibet in relatione ad centrum circulationis. Cum hæc duo diversa centra confuderit, in fundamentalibus his propositionibus, super quibus calculum suum struebat; non potuit fieri, quin in superedificando peccaret: neque ex erroribus illis extricare se valuit, per ignorantiam suam in secundis tertiisque differentiis. Atque hoc ulterius constat, ex sexto secundi tractatus articulo. Quippe in isto articulo lapsus est Leibnitijs; peccatumque eo admisit, quod nesciret secundas tertiasque differentias recte tractare. Cum hos itaque tractatus componeret, in discipulorum adhuc classe versabatur; idque eum decet, si pudor est, candidè fateri.

Omnino ergo verisimile est, quemadmodum ex dictis tribus Newtoni epistolis, cum Barroviana tangentium methodo comparatis, differentialem illam methodum extuderat Leibnitijs; ita decennio post, cum Newtoni Principia Philosophiæ in publicum prodirent, aliquatenus eum in illa progressum esse; dum tentaret dictam methodum ad primarias Newtoni propositiones extendere; & eâ occasione tres illos tractatus conficeret. Quippe propositiones, quæ in illis habentur, si errores & quicquid dempseris, omnino vel Newtonianæ sunt, vel ut corollaria ex eis facile deducendæ; in aliâ scilicet verborum formâ, re tamen non diversâ, jam antè à Newtono publicatæ. Has tamen Leibnitijs venditavit, tamquam à se solo diu antè inventas; quam à Newtono hæc editæ. Nempe in extremo primi tractatus, se invenisse eas fingit, antequam Newtoniana Principia prodirent; immo nonnullas ex eis, antequam ipse Parisiis discessisset, hoc est, ante Octobrem anni 1676. Tractatum autem secundum claudit his verbis: *Multa ex his deduci possunt praxi accommodata; sed nobis nunc fundamenta geometrica jecisse suffecerit, in quibus maxima consistebat difficultas: & fortassis, attentè consideranti, vias quasdam novas, satis antea impeditas, aperuisse videbimur. Omnia autem respondent nostræ Analysisi infinitarum; hoc est calculo summarum & differentiarum, cujus elementa quædam in his Actis dedimus, communibus quoad*

licuit

licuit verbis his expressis. In his, ut vides, jactat Leibnitijs, se primum fundamenta geometrica, in quibus maxima consistebat difficultas, in hoc ipso tractatu secundo posuisse; seque solum vias quasdam novas satis antea impeditas in tractatu eodem aperuisse: cum tamen prius fermè biennio prodissent Newtoni Principia, atque in hoc ipso tractatu edolando, subsidio fuissent Leibnitijs; quin & communibus quoad licuit verbis composita essent; atque omnia ista fundamenta omnesque istas vias novas in se contingerent. Atque horum omnium conscius erat Leibnitijs, tum cum tractatum illum ederet; ultroque tum agnoscere & prædicare debebat, Newtonum fuisse, qui *Fundamenta Geometrica in quibus maxima consistebat difficultas* primus posuerit; qui *vias novas, satis antea impeditas*, primus expediverit. Atque hæc quidem omnia quodammodo agnoscebat in responso ad D. Fatium; *Quam methodum*, inquit, *ante dominum Newtonum & me, nullus quod sciam geometra habuit; uti ante hunc, maximi nominis geometram, NEMO, SPECIMINE publicè dato, se habere PROBAVIT.* Atqui quod eâ occasione tam liberè fassus est Leibnitijs; si candor, si honor ei constet, ubique ac semper profiteri debet.

In epistolâ suâ 28 Maii 1697, ad Wallisium scriptâ, sic narrat N° LXXVI. Leibnitijs: *Methodum, inquit, fluxionum profundissimi Newtoni cognatam esse methodo meâ differentiali, non tantum animadverti, postquam opus ejus [Principiorum scilicet] & tuum prodit; sed etiam professus sum in Actis Eruditorum, & alias quoque monui. Id enim candori meo convenire judicavi, non minus quàm ipsius merito. Itaque communi nomine designare soleo Analyseos Infinitesimalis; quæ latius quàm tetragonistica patet. Interim quemadmodum & Vietæ & Cartesiana methodus Analyseos Speciosæ nomine venit, discrimina tamen nonnulla supersunt; ita fortasse & Newtoniana & Mea differunt in nonnullis.* Et in his quoque profitemur Leibnitijs, cum Newtoni Principia prodissent, se percepisse statim affinitatem, quæ inter geminas methodos intercedit; & idcirco communi se utramque infinitesimalis methodi nomine vocitare; quin & candoris sui esse, ut affinitatem illam agnoscat. Atqui si pro homine candido haberi se postulat; idem hoc, quod agnovit olim, & nunc debet agnoscere. Quin & fatetur methodum Newtonianam eo ferè gradu suæ methodo prævisse, quo Vietæam Cartesianæ:

fianæ: atque ut inter has, sic inter suam & Newtoni, discrimina quædam manere: & deinde ea enumerat, quibus methodum Newtoni ampliâsse, & promovisse se arbitratur. Atqui temporis illam prærogativam, quam tum; apud Wallisium, Newtono concedebat; etiam adhuc, & apud suos, debet concedere.

Cum discrimina illa, sive augmenta, Newtoni methodo à se ad-dita memorat Leibnitius; in secundo loco ponit Differentiales Æquationes. Atqui epistolæ illæ, quæ anno 1676 inter ipsos intercesserunt, clarè monstrant Newtonum eo tempore differentiales istas habuisse; Leibnitium verò minimè. In tertio loco recenset

Æquationes Exponentiales: atqui has quoque Anglis debet Leibnitius. Wallisius, in serierum interpolatione, consideravit fractos & negativos dignitatum indices: Newtonus in computationes analyticas fractos^(a), furdos, negativos, & indefinitos dignitatum

Nº LXIV. indices introduxit; & epistolâ 24 Octob. 1676 certior fecit Leibnitium, ad affectas æquationes, quæ dignitates eas, quarum indices fracti vel furdi erant, involverent, suam methodum se extendere. In responso autem, 21 Jan. 1677 dato, vicissim petit à

Nº LXIX. Newtono Leibnitius, ut dicere vellet quid de resolutione æquationum sentiret, involventium dignitates quarum indices essent indeterminati; quales hæc essent, $x^x + y^y = xy$. $x^x + y^y = x + y$. Atqui has ipsas æquationes nunc exponentiales nominat; seque orbi literato venditat primum earum inventorem; hancque ut eximiam quandam inventionem ostentat: nec tamen vel agnovit hæcenus auxilia, ad rem eam inveniendam, à Newtono sibi subministrata; nec vel uno exemplo utilitatem ejus, ubi dignitatum indices sint fluentes, ostendere valuit. Cum autem, ut credibile est, nondum eam, pro solitâ suâ impatientiâ, præque talis exempli inopiâ abjecerit; æquum est, ut speremus tandem eum aliquando mirificam ejus utilitatem publicè ostensurum esse.

Nº LXIV. In epistolâ ad Leibnitium, 24 Octob. 1676 datâ, Newtonus dixerat, se binas, inversa tangentium problemata & alia ejusmodi difficilia resolvendi, methodos habere; quarum unam consistere in assumendo seriem pro quavis ignotâ quantitate, unde cetera commodè possent deduci; & in conferendo homologos terminos suborientis æquationis, pro determinandis assumptæ seriei terminis. Quid hæc

(a) Vide Tom. I. p. 19, Not. b.

facit

facit Leibnitius? Multos post annos, in Actis scilicet Eruditorum Augusti 1693, hanc methodum tamquam suam publicat, ejusque primam sibi inventionem arrogat. Aut publicè verò huic abrenuntiet; aut argumentis vincat se eam invenisse, priusquam dictas Newtoni literas acceperit.

Illud quoque publicè ei confitendum est; se Oldenburgii epistolam 15 Aprilis 1675 accepisse; quâ plurimæ series convergentes pro Curvis quadrandis, & imprimis illa Jacobi Gregorii pro arcu dati tangentis inveniundo, atque inde circulo quadrando, continebantur. Hoc quidem privatim fassus est, epistolâ ad Oldenburgium, propriâ manu 20 Maii 1675 scriptâ, quæque etiam ad huc superest in libro Regiæ Societatis epistolari: nondum tamen publicè agnovit; ut tum sane factum oportuit, cum illam ipsam seriem ut suam maluit edere.

Porro illud quoque agnoscendum ei est publicè; se extracta Nº XLVII. epistolarum Gregorianarum accepisse, quæ ipsius rogatu Parisios ei misit Oldenburgius mense Junio 1676; in quibus erat una Gregorii, de istâ serie 15 Feb. 1671; & Newtoni alia, de Methodo Fluxionum, 10 Decemb. 1672.

Quandoquidem autem in epistolâ, 28 Dec. 1675, Oldenburgio significavit Leibnitius, se seriem illam cum amicis Parisiensibus, biennio antè, communicâsse; deque eâ re aliquoties ad ipsum scrip- Nº XLIV. sisse: in aliâ item epistolâ, 12 Maii 1676, se de serie illâ ante ali- Nº LII. quot annos ad ipsum literas dedisse: porro in aliâ, 27 Aug. 1676, se seriem illam amicis ostendisse triennio antè & ampliùs, hoc est, quàm primum Parisios à Londino venisset: illud à Leibnitio jure expectamus, ut dicat quâ evenerit, ut, cum Oldenburgii epistolam Nº XLII. 15 Apr. 1675 acciperet, illam ipsam seriem esse suam ignoraverit.

In epistolis, 15 Jul. & 26 Octob. 1674 datis, non nisi unam Nº XXXII. seriem memorat Leibnitius pro circuli circumferentiâ; metho- XXXIII. dumque, quâ ad hanc pervenerit, sibi etiam seriem obtulisse dicit, pro arcu cujus sinus datus fuerit, etsi arcus proportio ad circumferentiam totam sit ignota. Ergo ista methodus, ex dato triginta graduum sinu, seriem ei suppeditavit pro circumferentiâ totâ. Quòd si seriem quoque habuit pro totâ circumferentiâ, à XLV graduum tangente deductam; rogatur ut publicè doceat, quâ methodo, quæ ambas istas series ei dare posset, eo tempore sit usus:

usus: cum methodus per figurarum transmutationem nequaquam hoc efficere valeat. Rogatur insuper, ut rationem reddat, cur in istis epistolis non nisi unam circuli quadraturam memoret.

Porro si anno 1674 jam tum demonstrationem habuit seriei pro inveniendis, cujus sinus datus sit, arcu; rogatur, ut eam in publicum proferat; dicatque, cur in epistolâ 12 Maii 1676 ab Oldenburgio peteret, ut ille Newtoni demonstrationem pro ipsâ illâ serie à Collinio adipisceretur; & quâ tandem re Newtoniana series à suâ illâ differat. Quippe ex his omnibus non levis suspicio oritur, Newtonianam seriem, pro reperiendo cujus sinus datus sit arcu, Leibnitio dum in Angliâ commoraretur, esse traditam: illamque postea, 1673, Parisiensibus eam amicis pro suâ vendidisse; proximoque anno, etiam ad Oldenburgium, quasi de suâ literas dedisse, quo demonstrationem, five methodum serierum ejusmodi inveniendarum, expiscaretur. Anno verò insequente, cum Oldenburgius & istam, de quâ loquimur, seriem, & illam Gregorianam, & sex præterea alias ad ipsum misisset; non diutius eam seriem arrogare sibi Leibnitius sustinuit, inopiâ demonstrationis: seque dixit, series istas lentè examinare, cumque suis comparare; quasi suæ illæ à seriebus ex Angliâ missis essent diversæ. Denique, cum seriei Gregorianæ demonstrationem extudisset per figurarum transmutationem, Parisiensibus eam seriem amicis velut suam ostentare cœpit; ut ipse in Actis Eruditorum Apr. 1691, pag. 178 narrat; *Jam anno, inquit, 1675 compositum habebam opusculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum, &c.* At amicos istos celavit epistolam, quâ per Oldenburgium illam seriem nactus est; ipsique adeo Oldenburgio asseveravit, se, uno alterove anno ante scriptam ejus epistolam, seriem istam repperisse. Porro & sequente anno, cum binas Newtoni series per Gregorium quendam Mohr iteratò accepisset, sic de eis ad Oldenburgium scripsit, ut numquam sibi antea visis; ab eoque petiit, ut, per Collinium, Newtoni, pro eis inveniendis, methodum nancisceretur. Cæterum hanc gravem suspensionem si eluere volet Leibnitius; illud imprimis argumentis certis ostendat, se seriem istam Gregorianam, priusquam per Oldenburgium eam accepisset, suo solius acumine repperisse.

Hoc

Hoc quoque, prout æquum est, monstrabit Leibnitius; quâ primùm methodo diversas illas regressionis series, pro circulo & hyperbolâ, à Newtono quidem ad ipsum missas 13 Jun. 1676, at in epistolâ suâ 27 sequentis Augusti sibi attributas, invenerit, antequam à Newtono eas accepisset.

Cumque ab ipso rogatus Newtonus regressionis methodum ei indicavisset; quâ ut primùm legit Leibnitius, neque suam esse agnovit, & ne intellexit quidem: postea verò quâ percipere eam potuit, ut suam sibi arrogavit, olim scilicet à se inventam, sed in chartis suis reconditis oblivione sepultam: vel probet Leibnitius, si candidi æquique hominis nomen cupit auferre, se primùm ejus methodi inventorem esse, vel vero inventori concedat. In literis ad Oldenburgium datis 3 Feb. 1677, proprietatem quandam seriei numerorum, naturalium, triangularium, pyramidalium, triangulo-triangulorum, &c. ut inventum suum ostendit Leibnitius; quoque majorem fidem faceret, mirari visus est D. Pascalium, in triangulo suo Arithmetico, eam præterisse. Cæterum is liber Pascalii anno editus est 1665, atque istam ipsam seriei ejus proprietatem continet. Agnoscat itaque Leibnitius, proprietatem istam minimè à Pascaliò fuisse præteritam; neque pergat sibi vindicare, cum veri inventoris injuriâ. Abrenuntiet quoque methodo differentiali Newtoni; neque se in partes ingerat, quasi secundus scilicet inventor. Secundis inventoribus, etiam reverâ talibus, vel exiguis vel nullus est honos; tituli vel juris nihil est. Quid cum istis igitur fiet, qui vel secundos se fuisse, nullis certis argumentis possunt evincere? In literis ad D. Sloane, 29 Decemb. 1711 datis, amicos ait suos probè scire, quo pacto differentialem methodum invenerit. Quid amicos nobis narrat? Ipse planè, aperte, sine tergiversatione dicat, quâ eam viâ repperit.

In iisdem ad D. Sloane literis narrat, se, novennio ante quâ eam in lucem ederet, methodo potitum esse; hoc est, anno 1675 vel prius. Atqui certum est, 27 Aug. 1676, cum literas ad Oldenburgium mitteret, nondum illum habuisse eam. Quippe ibi affirmat, problemata inversæ tangentium methodi, plurimæque alia, non posse ad series infinitas, neque ad æquationes, aut qua-

draturas

draturas reduci. Quomodo hæc duo conciliari inter se possint, ipse, ubi otium est, videbit.

Jam supra didicimus, Leibnitium, dum per Angliam & Hollandiam domum rediret, dedisse operam Slusianæ pro tangentibus methodo promovendæ, & ad omne genus problemata extendendæ; eaque causâ generalem tangentium tabulam conficere voluisse. Nondum igitur veram istius methodi promotionem invenerat. Atqui semestri ferè post tempore, cum in veram ejus promotionem recens inciderat, rescripsit his verbis: *Clarissimi Slusii methodum tangentium nondum esse absolutam, celeberrimo Newtono assentior: & jam a multo tempore rem tangentium generalius tractavi, scilicet per differentias ordinarum.* Bene sanè, à multo tempore; nimirum semestri. Excogitet jam aliquid, pro candore suo, Leibnitius, cur tantillum temporis ut multum deprædicaverit; nisi eo consilio, ut inventoris titulum Newtono præiperet; fidemque faceret, se, diu antequam Newtoni literis eam edoctus esset, differentialem methodum penes se habuisse.

In Actis Eruditorum Junii 1696, dum duos priores Wallisianorum Operum tomos recensent editores (hoc est, ipse Leibnitius) ita narrant: *Ceterum ipse Newtonus, non minus candore quam præclaris in rem mathematicam meritis insignis, publicè & privatim agnovit Leibnitium, tum cum, interveniente celeberrimo viro Henrico Oldenburgio Bremensi Societatis Regiæ Anglicanæ tunc secretario, inter ipsos, ejusdem jam tum Societatis socios, commercium intercederet, id est, jam ferè antè annos viginti & amplius, calculum suum differentialem, seriesque infinitas, & pro iis quoque methodos generales habuisse; quod Wallisius, in præfatione operum factæ inter eos communicationis mentionem faciens, præterit; quoniam de eo fortasse non satis ipsi constabat. Ceterum differentiarum consideratio Leibnitiana, cujus mentionem facit Wallisius, nequis scilicet, ut ipse ait, causaretur de calculo differentiali nihil ab ipso dictum fuisse, meditationes aperuit, quæ aliunde non æquè nascebantur.* Ex his patet à Leibnitio lectam esse præfationem illam Wallisii; in quâ narrat is, Newtonum, anno 1676, methodum suam fluxionum Leibnitio explicavisse; quam tamen decennio antè, vel amplius, Newtonus invenisset. Atqui à Newtono nunquam creditum est, Leibnitium ante annum 1677 differentialem methodum

dum invenisse: ipseque adeo Leibnitius, in Actis Eruditorum Apr. 1691, p. 178, fassus est, inventam esse postquam domum Parisiis redisset ad negotia publica capeffenda; hoc est, post annum 1676. Quod autem ad generalem ejus infinitarum serierum methodum attinet; tantum abest ut generalis dicenda sit, ut vel exiguæ vel nullius prorsus sit utilitatis; nisi fortè ut ansam Leibnitio præbeat, quâ Gregorianam, pro circulo quadrando, seriem sibi adhamet transferatque.

In responso ad D. Fatium, in Actis Erud. 1700, p. 203, edito, hæc habet Leibnitius. *Ipse [Newtonus] scit unus omnium optimè, satisque indicavit publicè, cum sua Mathematica Naturæ Principia publicaret, anno 1687, nova quædam inventa geometrica, quæ ipsi communia mecum fuere, NEUTRUM LUCI AB ALTERO ACCEPTA, sed meditationibus quemque suis debere; & à me decennio antè [i. e. anno 1677] exposita fuisse.* Atqui in libro Principiorum hîc ad partes vocato, minimè agnovit Newtonus, suis eam methodum viribus invenisse Leibnitium, non à Newtonianis illis epistolis adjutum: Wallisiusque nuper contrarium asseveraverat, resellente tum nemine, vel contradicente. Quod si postea eam sine ope Newtoni quàm maximè invenisset Leibnitius, secundis tamen inventoribus exilis prorsus est gratiâ, nec nisi in inferiori subsellio locus; ne dicam, jus omnino nullum.

In eodem ad Fatium responso hæc quoque habet Leibnitius: *Certè cum elementa calculi mea edidi anno 1684, ne constabat quidem mihi aliud de inventis ejus [sc. Newtoni] in hoc genere, quàm, quod ipse olim significaverat in literis, posse se tangentes invenire non sublatis irrationalibus; quod Hugenus quoque se posse mihi significavit postea, etsi cæterorum ejus calculi adhuc expers. Sed majora multo consecutum Newtonum, viso demum libro Principiorum ejus, satis intellexi.* In his iterum agnovit, librum Principiorum ad Newtonianam fluxionum methodum sibi aditum patefecisse: idem tamen ipse jam negat, quicquam illius methodi in dicto libro contineri. In his simulat, se, priusquam iste liber prodisset, nihil amplius de Newtoni inventionibus scivisse, quàm quod methodum quandam tangentium habuerit; & ex isto demum methodum ejus fluxionum percepisse: atqui in epistola, 21 Jun. 1677 data, agnovit methodum eam ad curvilinearum figurarum quadraturas

N^o LXVI. draturas se extendere, suæque similem esse. Verba ejus hæc sunt: *Arbitror quæ celare voluit Newtonus, de tangentibus ducendis, ab his non abluere. Quod addit, ex hoc eodem fundamento quadraturas quoque reddi faciliores, me in sententia hac confirmat; nimirum semper figuræ illæ sunt quadrabiles, quæ sunt ad æquationem differentialem.*

N^o XXVI,
XLVI,
XLVIII,
LIV.

Newtonus in tribus illis epistolis, quas, ut diximus, ab Oldenburgio Leibnitius acceperat, tam generalem esse suam methodum dixerat, ut ope æquationum, finitarum & infinitarum, determinaret maximas & minimas, tangentes, areas, solida contenta, centra-gravitatis, longitudines ac curvaturas curvarum linearum, curvilinearumque figurarum, idque sine ablatione radicalium; extenderetque se ad similia problemata in Curvis, ut vulgo vocantur, Mechanicis; itemque ad problemata tangentium inversa; & ad omnia ferè, nisi fortè numeralia quædam, qualia sunt Diophanti, Leibnitius verò, in epistolâ 27 Aug. 1676, vix credere se posse finxit, eam methodum tam esse generalem. Newtonus, in primâ ex tribus illis epistolâ, tangentium methodum proposuit, ex generali eâ methodo deductam, exemploque eam illustravit; generalisque methodi raram, vel corollarium, esse monuit; & ejusmodi esse Slusianam, quæ nondum tum prodierat, coniecit. Hæc re excitatus Leibnitius meditabatur, siquâ viâ promovere posset methodum Slusianam, eamque ad omnia problemata extendere; quemadmodum jam antè ex ejus literis monstravimus. In tertiâ verò epistolâ suam methodum illustraverat Newtonus per theoremata pro quadraturis, & eorum exempla. Quibus adjutus Leibnitius, in epistolâ 21 Junii 1677, methodum suam cum Newtonianâ congruere dixit, ducendo tangentes; producendo methodum Slusii; procedendo sine fractionum & furdarum ablatione; quadraturasque reddendo multò expeditiores. His tot & toties actis, ad conterraneos suos affirmare, se, cum differentialem methodum anno 1684 ederet, nihil tum amplius de Newtoni invento inaudivisse, quàm quòd is methodum quandam tangentium haberet, cujus tandem est hominis?

Vide A^{ca}
Lauditorum
pro me: Je
Nov. 1684.

Porro eo tempore Leibnitius de suâ methodo nihil aliud explicaverat, nisi per eam tangentes duci posse, maximasque & minimas determinari, sine ademptione fractionum vel furdarum. Hoc verò

verò totum etiam per Newtoni methodum effici posse, certo sciebat; neque candidi erat hominis id dissimulare. Cum autem hactenus suam methodum exposuisset Leibnitius, addidit, se hanc geometriæ multo sublimioris initia posuisse, pervenientis ad difficillima quæque & utilissima problemata, quæ sine calculo differentiali, AUT SIMILI, vix solvi possint. Quid verò illud AUT SIMILI sibi vellet, quæ quæso conterranei ejus, sine CEdipode, poterant intelligere? Enimvero planis disertisque verbis dictum ab eo oportuit, SIMILEM illam, quam innuit, methodum Newtoni fuisse; quàm latè ea pateret, quàm à longo tempore reperta esset, prout ipse ex Angliâ didicisset, narrare; suamque illâ posteriorem esse confiteri. Hoc omnes controversias præcidisset; hoc candidi & honesti viri officium erat. Horum tamen omnium quasi oblitus, suis ille conterraneis, in responso ad Fatium, prædicat, se cum anno 1684 calculi sui elementa ederet, nihil tum de ullâ SIMILI methodo inaudivisse; nihil de ullâ aliâ nisi ad ducendas tangentes: quod qualis hominis fuerit, aliis dicendum relinquo.

Illud denique Leibnitio est expediendum: quæ factum sit, ut in responsis suis ad Wallisium & Fatium, quorum uterque primi ejus methodi inventoris gloriam Newtono detulerat, nihil tum ipse de se, ut priore inventore, dicebat; sed senum geometrarum mortem operiebatur, aliosque, qui superstites adhuc sunt, pro novitiis habebat: quin & ipsum Newtonum adortus, cum quoquam se alio certaturum negabat. Atqui dixerat ei Newtonus, in epistolâ 24 Octob. 1676, se tum ante annos quinque, quo quietius N^o LVII. ætatem ageret, consilium publicandi, quæ de hoc argumento scriberat, abjecisse: & ex eo quidem tempore studiose vitavit omnes, de rebus philosophicis ac mathematicis, disputationes; quin & à commercio de his rebus literario, ut disputationibus ansam porrigente, datâ operâ abstinuit; eandemque ob causam, neque de Leibnitio queri prius sustinuit, quàm in Actis se Lipsiensibus ut plagiarium traduci vidisset; Keilliumque eo tantum nomine in lites trahi, quòd ab hoc eum crimine vindicare conatus sit.

Insimulatum quidem est, quasi Regia Societas sententiam contra Leibnitium in hac causâ tulisset, non utrâque parte auditâ. Non ita se res habet. Nondum sententiam tulit Societas. Leibnitius quidem postulabat à Societate, ut Keillium inauditum damnare

nare vellet: adeo ut ipse jure eodem sic damnari potuisset; cum idem sit jus Seio quod Titio, Keillio quod Leibnitio. Cumque accusationem suam adversus Keillium distinuisset Leibnitius, jure potuisset Societas notam illi inurere. Ea verò certorum tantum hominum confessum legit, qui scrutarentur epistolas atque chartas, quæ de his rebus in Archivis Societatis habentur; & secundum illas chartas epistolasque rem ipsam, ut erat, Societati narrent. Non enim ideo lecti erant, ut Leibnitium vel Keillium, sed ut veteres chartas, examinarent: in eaque re probè se & honestè gesserunt. Numerosus quippe confessus erat; è viris eruditis diversarum nationum lectus: quorum fidem in epistolis chartisque examinandis, fideliterque edendis, nihil quicquam, ullius hominis gratiâ, addendo, vel omittendo, vel mutando, Societas tota comprobavit. Quin & ipsæ epistolæ atque chartæ, Societatis jussu, conservantur adhuc; ut si quis velit, ibi consuli, & cum edito Commercio Epistolico comparari possint. Illud interim submonendus est Leibnitius; cum id Societati impingit, quasi inauditum eum condemnatum isset, id ob eam rem, per statutum ejus quoddam, commeritum se esse, ut nomen ejus inde expungatur.

Philosophia porro, quam in Principiis suis atque Opticis Newtonus excoluit, est experimentalis: illa scilicet, quæ causas rerum non fidentius docet, quam per experimenta confirmari queant; neque implenda est opinationibus, quæ per phænomena nequeunt probari. Et idcirco in Opticis suis, res experimentis firmatas, ab illis quæ incertæ adhuc manent, distinxit Newtonus: & incertas aliquot ejusmodi, sub finem Opticorum, ut quaerenda proposuit. Eandemque ob causam, in Principiorum præfatione, cum memorasset motus planetarum, cometarum, lunæ ac maris, ceu in libro illo de Gravitatis theoriâ deductos, hæc addidit: *Utinam cætera naturæ phænomena ex Principiis Mechanicis, eodem argumentandi genere, derivare liceret. Nam multa me movent, ut nonnihil suspicer, ea omnia ex viribus quibusdam pendere posse, quibus corporum particule, per causas nondum cognitæ, vel in se mutuo impelluntur, & secundum regulares figuras coherant, vel ab invicem fugantur, & recedunt: quibus viribus ignotis, philosophi hactenus naturam frustra tentarunt.* Et sub finem ejus libri, in se-

cunda

cunda editione, narrat; ut, præ inopiâ experimentorum tanto negotio sufficientium, non aggressus sit leges actionum illius spiritus, sive agentis, describere, per quem efficitur hæc attractio. Quin & eandem ob causam de Gravitatis causâ nihil pronuntiat; quod nulla experimenta, sive phænomena, ad manum essent, quæ causam illam certò indicare possent. Atque hoc in Principiis suis, sub ipso initio, abunde declaraverat, his verbis: *Virium causas & sedes physis jam non expendo.* Et paulo post: *Voces attractionis, Impulsus, vel Propensionis cujuscunque in centrum, indifferenter & pro se mutuo promiscue usurpo; has vires, non physis, sed mathematicè tantum considerando.* Unde caveat lector, ne per hujusmodi voces cogitet me speciem, vel modum actionis, causamve aut rationem physicam alicubi definire; vel centris, quæ sunt puncta mathematica, vires verè & physis tribuere; si fortè aut centra trahere, aut vires centrorum esse dixerò. Et sub finem Opticæ: *Quæ causâ efficiente hæ attractiones* [sc. gravitas, visque magnetica & electrica] *peragantur, hic non inquirò.* Quam ego, attractionem appello, fieri sane potest, ut ea efficiatur impulsu; vel alio aliquo modo nobis incognito. Hanc, vocem attractionis ita hic accipi velim, ut in universum solummodo vim aliquam significare intelligatur, quâ corpora ad se mutuo tendant; cuiusque demum causæ attribuenda sit illa vis: nam ex phænomenis naturæ illud nos prius edocitos esse oportet, quænam corpora se invicem attrahant, & quænam sint leges & proprietates istius attractionis, quàm in id inquirere par sit, quænam efficiente causâ peragatur attractio. Pauloque inferius, easdem attractiones tamquam vires considerat, quas in rerum naturâ existentiam habere, licet causas earum nondum sint cognitæ, per phænomena constat: distinguitque eas à qualitatibus occultis, quæ à specificis rerum formis fluere existimantur. Et in scholio sub extremum Principiorum; cum Gravitatis proprietates memorasset, hæc addidit: *Rationem verò harum Gravitatis proprietatum, ex phænomenis nondum potui deducere; & hypotheses non fingò.* Quicquid enim ex phænomenis non deducitur, hypothesis vocanda est; & hypotheses, seu metaphysicæ, seu physis, seu qualitatium occultarum, seu mechanicæ, in philosophiâ experimentalis locum non habent. Satis est, quid Gravitatis reverà existat, & agat secundum leges à nobis expostas, & ad corporum celestium & maris nostri nos omnes

omnes sufficiat. Jam verò, post hæc omnia, quæ consulto præmonuerat Newtonus, quis non miretur, ideo eum à quoquam fugilari, quòd causas gravitatis aliarumque attractionum non per hypotheses explicet? quasi criminis loco esset, certis esse contentum, incerta verò dimittere. Et tamen Actorum Eruditorum (anno 1714 mense Martio, p. 141, 142) editores, id Newtono exprobrant, quòd causam Gravitatis neget esse mechanicam; afferuntque, si spiritus ille, vel agens, quo electrica fit attractio, non sit æther vel subtilis Cartesii materia, quâvis id hypothesei contemnitius esse; ut fortasse sit Principium Henrici Mori hylarchium. Quin & ipse Leibnitius, in tractatu *De bonitate Dei*, & in epistolis ad Hartsoekerum atque alibi, Newtono id vitio vertit, quasi Gravitatem faceret naturalem quandam & essentialem corporum proprietatem, immo occultam qualitatem, ac denique miraculum. Atque hujusmodi cavillationibus, homines hi conterraneis suis persuasum esse cupiunt, judicio eum & acumine parum valere; neque eum esse, qui methodum infinitesimalem, rem tam arduam, invenire potuisset.

Illud profecto confitendum est; in philosophiâ tractandâ Newtonum inter & Leibnitium plurimum interesse. Prior ille eo usque progreditur, quo phænomenorum & experimentorum evidentiâ eum ducit; & ubi illa deficit, pedem sistit: posterior hypothesibus suis scatet totus; easque proponit, non experimentis examinandas, sed clausis oculis credendas. Ille, inopiâ experimentorum, quæ causam Gravitatis certo indicare possint, utrum mechanica fuerit necne, non affirmat: hic, si mechanica non sit, perpetuum esse miraculum pronuntiat. Ille, atque id quoque non definiens sed quærens, Creatoris potentiæ tribuit, quòd minimæ quæque materiæ partes sint duræ: hic illam materiæ duri-
 tiem conspirantibus quibusdam môtibus imputat; &, si causa ejus alia ponatur quàm mechanica, pro perpetuo eam miraculo deridendam propinat. Ille motum in homine animale, non audet affirmare, merè esse mechanicum: hic purè mechanicum esse audacter asserit; cum ex hypothesei ejus de harmoniâ præstabilitâ, numquam anima vel mens hominis sic agat in corpus, ut motus hujus vel impediatur, vel adjuvet. Ille Deum asserit (*Deum in quo vivimus, & movemur, & sumus*) esse omnipræsentem; non
 tamen

tamen ut mundi animam: hic, non mundi quidem animam esse, sed INTELLIGENTIAM SUPRAMUNDANAM: ex quo illud consequi videatur, non posse Deum intra mundi limites quicquam efficere, nisi per miraculum prorsus incredibile. Ille philosophis præcipit, ut à phænomenis & experimentis ad eorum causas progrediantur; atque inde ad causarum istarum causas; & sic deinceps, donec ad primam causam perveniatur. Hic omnes causæ primæ actiones pro miraculis haberi; omnesque leges per Dei voluntatem naturæ impressas pro perpetuis miraculis occultisque qualitatibus censerent; & idcirco ex philosophiâ exulare jubet. Siccine verò agitur? An perpetuæ & universales naturæ leges, si ex potentiâ Dei, causæve adhuc nobis incognitæ actione deriventur, pro miraculis & qualitatibus occultis, hoc est ex ejus sententiâ, pro monstris & absurditatibus, sunt exhibendæ? Omnia porro pro Dei existentiâ de naturæ phænomenis sumpta argumenta, idcirco sunt explodenda, quia novis quis ea nominibus & ignominiosis infamet? An, ut superstitiosa & absurda, rejiciatur Philosophia Experimentalis, quia neque ultra experimenta definire quicquam vult; neque adhuc per experimenta probare potest, naturæ omnia phænomena per causas merè mechanicas posse solvi? Res profecto digna est, quæ & maturè & seriò consideretur.

COMMERCIUM
EPISTOLICUM
D. JOHANNIS COLLINS,
ET ALIORUM,
DE
ANALYSSI
PROMOTA:
JUSSU
SOCIETATIS REGIÆ LONDINENSIS, ANNO 1712,
IN LUCEM PRIMUM EDITUM.

A D

LECTOREM.

QUAM ob causam editæ sint hæ epistolæ chartulæque collectaneæ, apparebit ex literis D. Leibnitii & D. Keillii in fine subjunctis. Offensionem attulerant D. Leibnitio nonnulla, quæ scripto prodidit D. Keillius in Actis Londinensibus anno 1708, injuriam D. Newtono oblatam propulsans. Datis igitur ad Societatis Regalis secretarium literis, de calumniâ questus D. Leibnitius, remedium à Societate petiit; idque eos æquum credidit judicaturos, ut D. Keillius culpam suam publicè fateretur. D. Keillio ea est pars visa potior, ut ad illa, quæ questus erat D. Leibnitius, literis scriptis responderet: quibus in literis, quæ antea ediderat, & exposuit plenius, & vindicavit. D. Leibnitius, nequaquam bis satis sibi factum arbitratus, literas alteras ad Societatem dedit; in quibus adhuc de D. Keillio questus, novum eum hominem appellat, parumque peritum rerum anteactorum cognitorem; nec mandatum ab eo, cujus interesset, habentem; Societatisque æquitati committit, annon coercendæ sint vanæ & injustæ vociferationes.

Versabatur in Angliâ D. Leibnitius ineunte anno 1673, iterumque mense Octobri 1676; & interjecto illo temporis intervallo in Galliâ egit. Quo omni temporis spatio, mutuis acceptis datisque literis, commercium habuit cum D. Oldenburgo; & Oldenburgi operâ, tandem cum D. Collinio itidem, & nonnunquam etiam cum D. Newtono. Quid autem ille ex Anglis tandem, vel tum cum Londini esset, vel ex literis istis mutuò datis, edidicerit, in eo forè vertitur hæc omnis questio. D. Oldenburgus & Collinius jam diù obierunt. D. Newtonus autem tum Cantabrigiæ egit; parumque amplius novit, quàm quod ex literis ipsius à D. Wallisio deinceps editis apparet. D. Newtonus neque à D. Keillii partibus testis esse potest, nec D. Leibnitius ipse à suis: alius autem in vivis testis est nullus. Societas itaque regalis, à D. Leibnitio bis adversus Keillium appellata,

lata, selectorum ex Societate arbitrorum consessum constituit; quæ literas, literamque transcriptarum libellos, aliasque chartulas à D. Oldenburgo penes Societatem relictas, & siquid inter D. Collinii schedas repertum buc faceret, perscrutarentur, sententiamque suam ad Societatem referrent: jussitque tandem ut sententia illa, à selectorum arbitrorum consessu relata, unâ cum ipsis literarum, aliarumque chartularum excerptis, emitteretur.

De hac methodo, ex methodis Serierum et Fluentium compositâ, scripsit infra Newtonus.
Nº VI, VII, XXII, XLVII, LIII, LVI.

Cum D. Newtonus *Analysin* istam scripto traderet, quæ sub initium borum collectaneorum impressa est, habuit jam tam * *methodum generalem equationes finitas in infinitas resolvendi, & equationes tum finitas tum infinitas applicandi ad problemata solvenda, ope proportionum augmentorum momentaneorum quantitatum nascentium, & augescentium.* Augmenta hæc appellat D. Newtonus *particulas, & momenta*; D. Leibnitijs autem *infinitesimales, indivisibiles, & differentias.* Quantitates augescentes appellat D. Newtonus *Fluentes*; D. Leibnitijs autem *Summas.* Et velocitates augmenti appellat D. Newtonus *Flaxiones*; istasque *fluxiones* exponit per *quantitatum fluentium momenta.*

Quæ pars hujus methodi in eo sita est, ut *equationes finitæ in infinitas resolvantur*, eam cum D. Leibnitio, rogatu suo, communicavit D. Newtonus, literis ad illum datis Junii 13, & Octobris 24, 1676. Reliquam hujus methodi partem, postquam eousque explicuerat, ut eam satis & obviam factam existimaret; ne sibi deinceps subriperetur, priusquam eam exponere visum foret, literis occultis ita celavit, quo modo aliàs Galileus atque Hugenius fecerant. Hujus posterioris partis inventionem sibi vendicat D. Leibnitijs: D. Keillius autem eam D. Newtono adserit; Keillioque suffragatur sententia selectorum ex Societate arbitrorum consessu. Alios tamen externos, qui methodum istam à D. Leibnitio acceperint, aut aliter obtinuerint, nihil quidquam in his collectaneis est, quod ullo pacto afficiat. Illi, quid inter D. Leibnitium & D. Oldenburgum commercii esset, ignorabant. Illis, quod methodum, quam utilem esse compererant, in rem suam adhibuerint, atque excoluerint; id verò laudi est dandum.

Subjunctæ sunt epistolis annotationes quædam; quod lectores, quibus minus est otii, & epistolas inter se facilius conferre, & semel perlectas intelligere queant.

C O M-

COMMERCIUM EPISTOLICUM

D. JOHANNIS COLLINS,

ET ITALIORUM,

DE ANALYSI PROMOTA:

JUSSU SOCIETATIS REGIÆ

IN LUCEM EDITUM.

Excerpta ex epistola reverendi viri D. Isaaci Barrow ad D. J. Collins, Can. N. L. tabrigiæ, 20 Junii 1669 datâ; cujus habetur Autographon.

AMICUS quidam apud nos commorans, qui eximio in his rebus pollet ingenio, nudiustertius chartas quasdam mihi tradidit, in quibus magnitudinum dimensiones supputandi methodos, Mercatoris methodo pro hyperbolâ similes, maxime verò generales, descripsit; simulque æquationes resolvendi: quæ, ut opinor, tibi placebunt; quas unâ cum proximis literis ad te mittam.

Ex epistola ejusdem ad eundem, 31 Julii 1669 datâ, pariterque ipse Barrovii manu scriptâ.

Mitto, quas pollicitus eram, amici chartas; quæ, uti spero, haud parum te oblectabunt. Remittas, quæso, quum eas quantum tibi visum fuerit perlegeris: id enim postulavit amicus meus, cum primum eum rogavi, ut eas tecum communicare mihi liceret. Quantocyus igitur, obsecro, te eas recepisse fac me certiorum; quod illis metuo; quippe qui eas per veredarium publicum ad te mittere non dubitaverim, quo tibi morem gererem quam citissimè.

Ex

Ex epistolâ ejusdem ad eundem, 20 Aug. 1669 datâ; cujus etiam comparet autographon.

Amici chartas tibi placuisse gaudeo; est illi nomen Newtonus, collegii nostri socius, & juvenis; secundus enim, ex quo artium magistri gradum cepit, jam agitur annus; & qui, eximio quo est acumine, permagnos in hac re progressus fecit. Illas, si vis, cum nobili domino vicecomite Brounkero communica.

Nº II. *Exemplar dictarum chartarum, manu D. Collins exaratum, & in scriniis ejus repertum, quod cum ipsius D. Newtoni autographo collatum ad verbum consentire invenimus. Hujus autem titulus est*

DE ANALYST PER ÆQUATIONES NUMERO TERMINORUM INFINITAS.

METHODUM generalem, quam de Curvarum quantitate—ex utraque parte lineæ BD. [Vide Tom. I. p. 257.]

Compositarum Curvarum Quadratura ex Simplicibus.

Nº III. REG. II. Si valor ipsius y , &c.—dabitur tota BD . [Vide Tom. I. p. 258—263.]

Aliarum Omnium Quadratura.

Nº IV. REG. III. Sit valor ipsius y , &c.— $\frac{1}{3}x^3$. [Vide Tom. I. p. 263, 264.]

Exempla Radicem extrahendo.

Nº V. Si Sit $\sqrt{aa+xx} = y$, &c.—superficies cognoscetur. [Vide Tom. I. p. 264—268.]

Exempla per Resolutionem Æquationum

Numeralis Æquationum affectarum Resolutio.

Nº VI. Quia tota difficultas—quæ quærebatur. [Vide Tom. I. p. 268—270.]

Literalis Æquationum affectarum Resolutio.

Nº VII. His in numeris sic— x sit minor. [Vide Tom. I. p. 270—272.]

Alius modus easdem Resolvendi.

Nº VIII. Sin valor areæ tanto—magis composita. [Vide Tom. I. p. 272.]

Nº IX. Sed hunc modum— $y = x^2 + 2x^3$, &c. [Vide Tom. I. p. 273, 274.]

Nº X. Et hæc de areis—in reliquis. [Vide Tom. I. p. 274—278.]

Applicatio

* N. B. Quadratura Curvarum per Æquationes infinitas, quæ nonnunquam terminantur & finitæ evadunt. Eadem explicatur in Prop. v. Libri de Quadraturis. Et propositio illa pendet à quatuor prioribus. Ideoque methodus fluxionum & momentorum, quatenus habetur in propositionibus

Applicatio prædictorum ad Curvas Mechanicas.

Et hæc de Curvis geometricis—(fiat) * exactè & Geometricè determinantur. Sed ista narrandi non est locus. Respicienti duo præ reliquis demonstranda occurrunt. [Vide Tom. I. p. 278—280.]

1. *Demonstratio Quadraturæ Curvarum Simplicium in Regulâ Primâ.*

Præparatio pro Regulâ Primâ demonstrandâ.

+ Sit itaque Curvæ—† Tum $x + o$ [Tom. I. p. 280]—§ possunt Nº XII. inveniri [Tom. I. p. 281]—minimarum dimensionum. [Tom. I. p. 282.]

Excerpta ex epistolâ D. Oldenburgh ad D. Renatum Franciscum Slusium Nº XIII. Canonicum Leodiensem, anno 1669, 14 Septembris St. vet. datâ; cujus apographum conspicitur in libro Societatis Regiæ, quo conservantur epistolæ, No. III. p. 174.

Insuper communicavit ille [Barrovius] universalem methodum analyticam, ipsi transmissam à D. Isaaco Newtono, inservientem mensurandis areis omnium ejusmodi Curvarum, & earundem perimetrorum, in quibus ordinatæ eandem habet communem habitudinem ad basin: hæcque methodus alia non est ab illâ, quam particulariter applicuit D. Mercator ad inveniendas areas hyperbolæ, universalis redditâ. Auctor sic incipit.

“ De Analyfi per Æquationes numero terminorum infinitas.

“ Methodum generalem, quam de Curvarum quantitate, per infinitam terminorum seriem mensurandâ, olim excogitaveram, &c.”

Et postquam ejus beneficio ostendit complurium Curvarum quadraturam, accedit ad circulum; & convertendo $\sqrt{aa+bb}$, vel $\sqrt{aa-bb}$ in seriem infinitam, ostendit complures ejusmodi series applicari posse ad circulum: adeo ut datis horum quibuscumque duobus; radio nempe, sinu, arcu, & areâ segmenti, reliquorum quodvis inveniri possit infinite verum: res ni fallor ab omnibus auctoribus prægressis valde expetita. Ejusdem etiam adminiculo eximie facilitavit inventionem radicis æquationis cujuslibet, & mediarum proportionalium; & seriem largitur ad inveniendam lineam Ellipticâ longitudinem. Similiter, ut ostenderet methodum suam ad Curvas Mechanicas, earumque tangentes, se porrigere, quadrat Cycloidem ejusque portiones; areamque curvæ Quadraticis, ejusque perimetrum invenit: atque ad calcem sic ait.

tionibus quinque primis Libri de Quadraturis, Newtono innotuit anno 1669.

† Exemplum luculentum calculi per momenta fluentium.

‡ Leibnitius scribit dx pro o vel ox ; dz pro oy , vel oy .

§ Hæc propositio ex æquatione fluentes involvente invenitur fluxionibus.

“ Nec

“ Nec quicquam hujusmodi scio, ad quod hæc methodus, idque variis
 “ modis, sese non extendat. Imo tangentes ad Curvas Mechanicas, si
 “ quando id non alias fiat, hujus ope ducuntur. Et quicquid vulgaris
 “ Analysis per æquationes ex finito terminorum numero constantes, quando
 “ id sit possibile, perficit; hæc per æquationes infinitas semper perficiat.
 “ Et hæc de arcibus Curvarum investigandis dicta sufficiant. Imo cum pro-
 “ blemata de Curvarum longitudine, de quantitate & superficie Solidorum,
 “ deque centro Gravitatis, possunt eo tandem reduci, ut quærat quantitas
 “ superficiæ planæ, lineæ curvæ terminatæ; non opus est quicquam de iis
 “ adjungere.”

N° XIV. Ex epistola D. Collins ad D. Jacobum Gregorium, anno 1669, 25 Nov.
 datâ. Quæ quidem epistola, manu dicti D. Collins descripta, conser-
 vata est.

Barrovius provinciam suam publicè prælegendi, nunc etiam, nomine
 Newtono, Cantabrigiensi; cujus, tanquam viri acutissimi ingenio præditi, in
 præfatione Prælectionum Opticarum, meminit: quique antequam ederetur
 Mercatoris Logarithmotechnia, eandem methodum adinvenerat; eamque ad
 omnes Curvas generaliter, & ad circulum diversimodè, applicat.

N° XV. Ex epistola D. Jacobi Gregorii ad D. J. Collins, ad Fano Sili. Andrea
 apud Scotos anno 1670, 20 Aprilis datâ, præsit in autographo ipsius Gre-
 gorii legitur.

Seriem à te missam de circuli zonâ intelligere nequeo, nempe $2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} - \&c.$ Si hæc rectè descripta sit, seriem legitimam non esse suspicor.

N° XVI. Ex epistola ejusdem Gregorii ad eundem, anno 1670, 5 Septemb. datâ.

Barrovii [geometricas] lectiones summâ cum voluptate & attentione per-
 legi; atque omnes, qui unquam hæc de rebus scripserunt, infinito intervallo
 superasse comperio. Ex ejusdem [Barrovii] methodis tangentes ducendæ
 cum quibusdam à propriis collatis, inveni methodum generalem & geome-
 tricam * ducendæ tangentes ad omnes Curvas sine calculo; & quæ com-
 plectitur non tantum Barrovii methodos particulares, sed & ipsius genera-
 lem methodum analyticam, quam habes sub finem lectionis decimæ. Me-
 thodus mea haud pluribus quam duodecim continetur propositionibus.

N° XVII. Ex epistola ejusdem ad eundem anno 1670, 23 Novemb. datâ; cuius etiam
 conservatur autographon.

Plurimæ approximationes pro circuli segmentis ex his faciliè elici pos-
 sunt; at vix operæ pretium erit, cum potestates alternas tollere nequeo,

* Hinc innotuit methodum tangentium Gregorii & Slusii ex methodo Barrovii consequi.
 quod

quod factum est à D. Newtono in suâ serie, modò series sit: nam, ut di-
 cam quod sentio, ad nullam mearum reducere possum. Autumno tamen
 meam pari facilitate & brevitate rem confecturam.

Ex autographo D. Jacobi Gregorii ad eundem D. Collins, de Fano Sili. An. N° XVIII.
 dreæ, 19 Decembris ejusdem anni, misso.

Quum postremas ad te dedi literas, nondum animadvertissem D. New-
 toni seriem de circuli zonis, quam jam dudum ad me misisti, quâ cum infi-
 nito istiusmodi serierum numero, confectarium illius esse posse, quam initi
 de logarithmis; nempe, dato logarithmo invenire ejus numerum; vel ra-
 dicem potestatis cujuscunque puræ in infinitam seriem permutare. Me sane
 tam tardi fuisse ingenii miror; qui tanto temporis spatio hoc non animad-
 verteram, quum tamen multum olei & operæ in istâ serie expiscandâ im-
 penderam. At ut ingenue fatear, semper in animum induxeram, si modò
 series esset, me in eam incidere posse, ope aliquarum è seriebus meis pro
 circulo inter se combinatis, quarum quidem plurimas ad manus habeo;
 neque ullam aliam desiderarâ methodum. Series tua, paululum producta,

fit $2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5R^9}{576R^7} - \frac{7B^{11}}{1408R^9} - \frac{21B^{13}}{6656R^{11}} - \frac{11B^{15}}{5120R^{13}} - \&c.$ Eisdem

etiam positis, erit arcus, cujus sinus B, = $B + \frac{B^3}{6R} + \frac{3B^5}{40R^3} + \frac{5B^7}{112R^5} + \frac{35B^9}{1152R^7} +$
 (*) &c. Plures hujusmodi series proferre possum; sed tu fortasse plus me-
 ipso de his rebus nôsti.

Ex epistola D. Collins ad dictam D. Gregorium, 24 Decembris, anno 1670 N° XIX.
 datâ: cujus habetur exemplar manu ipsius D. Collins descriptum.

Quum D. Dary * Miscellanea sua in lucem edidit, exemplar libelli misit
 ad D. Newtonum; qui dictum D. Dary serie, pro arcu zonæ circuli, quam
 ad te misi, remuneravit: quæ sine omni dubio series est legitima & eximia.
 Ope D. Barrovii nonnullas alias series, è methodo Newtoni generali derivatas,
 obtinui; easque, conferto colloquio, deprehendi analyticè deduci posse è da-
 tis cujuscvis figuræ proprietatibus; & multas series ad singulas figuras ap-
 plicari posse. Universalem quoque esse, cujuscque ope omnes Quadraturas
 perficere possis; tam Curvarum quas Cartesius Geometricas esse admittit,
 quam earum quas censet Mechanicas.

Hâc itaque methodo Curvæ omnium figurarum, communi proprietate de-
 finitarum, rectificantur; earum tangentes & centra gravitatis inveniuntur;
 item rotunda earum Solida, & segmenta secunda cubantur; & in universis
 Curvis, longitudine curvilineâ data, ordinatim applicatæ inveniuntur, &
 vice versâ.

Exempla quedam.

Arcu æ dato, invenire sinum x, vel co-sinum y; posita unitate pro radio.

(*) Vide Analysin per Æq. Num. Term. inf. Cap. III. § 6.

(*) Vide Analysin per Æq. Num. Term. inf. Cap. VI. § 3.

* N. B. Miscellanea edidit D. Michael Dary, anno 1669.

Vol. IV.

T t t

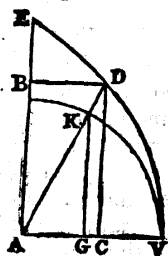
x =

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9 (*) - \&c.$$

$$y = 1 - \frac{1}{2}z^2 + \frac{1}{24}z^4 - \frac{1}{720}z^6 + \frac{1}{40320}z^8 - \frac{1}{362880}z^{10} (*) + \&c.$$

Et dato sinu x , invenire z : $z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 (*) + \&c.$

Quadratricem veterum quod attinet, nulla methodus, nullus geometra ejus aream exhibere valuit. Sit igitur AV, radius circuli inscripti, unitas; & VK, arcus, x ; erit area BDVA = $x - \frac{1}{2}x^2 - \frac{1}{24}x^4 - \frac{1}{720}x^6 - \&c. (*)$



Tractatum hæc de re scripsit; in quo inventio longitudinis totius, vel datæ partis, curvæ Ellipticæ, & Quadratricis DV, nec non areæ supradictæ, est inter exempla.

Nº XX. Ex epistola D. Jacobi Gregorii ad D. Collins, 15 Feb. anno 1677 datâ, cujus habetur autographon.

Ex quo epistolam ad te dedi, tres à te accepi, unam Decemb. 15, alteram Dec 24, tertiam 21 Januarii nuper elapsi datam.

Quod attinet Newtoni methodum universalem, aliquâ ex parte, ut opinor, mihi innotescit; tam quoad Geometricas, quàm Mechanicas curvas. Nihilo tamen minus, ob series ad me missas, gratias habeo; quas ut remunerem, mitto quæ sequuntur.

Sit radius = r ; arcus = a ; tangens = t ; secans = s .

$$\text{Et erit } a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8}, \&c. (*)$$

$$\text{Exitque } t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8}, \&c. (*)$$

$$\text{Et } s = r + \frac{a^2}{2r} + \frac{5a^4}{24r^3} + \frac{61a^6}{720r^5} + \frac{277a^8}{8064r^7}, \&c. (*)$$

Sit nunc tangens artificialis = t ; & secans artificialis = s ; & integer quadrans = q .

$$\text{Erit } s = \frac{a^2}{2r} + \frac{a^4}{12r^3} + \frac{a^6}{45r^5} + \frac{17a^8}{2520r^7} + \frac{62a^{10}}{28350r^9}, (*) \&c.$$

$$\text{Sit } 2a - q = e, \& \text{ erit } t = e + \frac{e^3}{6r^2} + \frac{e^5}{24r^4} + \frac{61e^7}{5040r^6} + \frac{277e^9}{72576r^8}, (*) \&c.$$

Sit nunc secans artificialis 45 gr. = s ; sitque $s + l$ secans artificialis ad libitum. Erit ejus arcus = $\frac{1}{2}q + l - \frac{l^3}{r} + \frac{4l^5}{3r^3} - \frac{7l^7}{3r^5} + \frac{14l^9}{3r^7} - \frac{452l^{11}}{45r^9}, (*) \&c.$ eritque $2a - q = t - \frac{t^3}{6r^2} + \frac{t^5}{24r^4} - \frac{61t^7}{5040r^6} + \frac{277t^9}{27576r^8}, (*) \&c.$

Hic animadvertendum est, radium artificialem esse 0; & ubi inveneris q majorem quàm $2a$, sive artificialem secantem 45 gr. majorem esse datâ secante; mutanda esse signa, & pergendum secundum vulgaris Algebrae præcepta.

(*) Vide Analysin per Aeq. Num. Term. inf. Cap. VII. Sect. 2.

(b) Vide Analysin per Aeq. Num. Term. inf. Cap. VI. § 2.

(c) Vide Analysin per Aeq. Num. Term. inf. Cap. IX. § 4.

Sit

Sit ellipsis, cujus alter semiaxium = r ; alter = c . Ex quolibet curvæ ellipticæ puncto demittatur in semiaxem, r , recta perpendicularis = a . Erit curva elliptica perpendiculari a adjacens = $a + \frac{r^2a^3}{6c^4} + \frac{4r^2a^5}{40c^6} + \frac{8c^4r^2a^7 + r^6a^9 - 4c^2r^4a^7}{112c^{12}} + \frac{64c^6r^2a^9 - 48c^4r^4a^9 + 24c^2r^6a^9 - 5r^8a^9}{1152c^{16}}, \&c.$

Si determinetur ellipsos species, series hæc simplicior evadet. Ut si $c = 2r$, foret Curva prædicta = $a + \frac{a^3}{96r^2} + \frac{3a^5}{2048r^4} + \frac{113a^7}{458752r^6} + \frac{1419a^9}{75457172r^8}, \&c.$

Reliquis verò manentibus, si Curva prædicta esset hyperbola, prædicta quoque series ei inferviret; si modò omnium terminorum partes affirmantur, & negentur totus terminus tertius, totus quintus, septimus, &c. in locis imparibus.

Gratias ago maximas, tam ob benevolentiam, quàm mones de meditatione meis publicandis, quàm ob perhumanas tuas pollicitationes. Nollem tantam molestiam tibi creare; neque mihi in animo est quicquam edere, præterquam quadraturam meam Circuli recusam, additis quibusdam nugamentis. Quod attinet methodum meam inveniendi radices omnium æquationum; una series unam tantum prodit radicem, at pro quolibet radice infinitæ sunt series. Industriâ autem aliquâ opus est, ad seriem ritè incipientem, & ad quam pertineat radicem dignoscendam. Verùm hæc de re fusius forsitan aliquando ad te scribam. Non est quod metuas cuiquam, quicquid mihi scribam, communicare; parum enim sollicitus sum, utrumne meo an alieno nomine in publicum prodeat.

Ex epistola D. Collins ad D. Bertet Parisiis tum agentem. Data autem est Nº XXII. 21 Februarii, anno 1700; ejusque exemplar, manu ipsius D. Collins exaratum, conservatur.

Systema Algebrae integrum componere, opus est eximium, & dignum cui ab omnibus faveatur; præcipue verò quia quatuor circiter abhinc annis inventa fuit à D. Isaaco Newtono methodus analytica generalis, pro quadraturâ omnium spatiorum curvilinearum, tam in Curvis Geometricis quàm Mechanicis, communi aliquâ proprietate gaudentibus. Hujus ope quicquid à quadraturis pendet peragitur; ut rectificatio Curvarum; inventio tangentium & centrorum gravitatis; rotundorumque Solidorum, & eorundem segmentorum secundorum, & curvarum superficierum dimensionum: non autem superficierum Solidorum quorum axes inclinantur; uti parabolicorum conoidum, &c. hæc manet difficultas posteris superanda. Hæc omnia peraguntur approximando verum in infinitum, absque radicem extractione, ope infinitæ seriei rationalium, cujusmodi multæ ad unam eandemque figuram diversimodè applicari possunt: v. g. ad Circulum una, ad invenientem aream totius, vel partis cujusvis; alia, ad inscriptas; alia, ad adscriptas; &c. Ita ut dato sinu, tangente vel secante, inveniri potest longitudo arcus, & vice versa, ope diversarum serierum ad eam rem appropriatarum.

(*) Vide Excerpt. ex Epist. Not. 1. § 7.

(b) Vide Excerpt. ex Epist. Not. 1. § 14.

(c) Vide Excerpt. ex Epist. Not. 1. § 23.

(*) Vide Excerpt. ex Epist. Not. 1. § 8.

(b) Vide Excerpt. ex Epist. Not. 1. § 22.

(c) Vide Excerpt. ex Epist. Not. 1. § 26.

Unde

Unde fit, ut jam calculo facilius inventu sit arcus è sinu dato, & vice versa, quàm è sinu dato sinus dupli arcus. Universum autem hoc nihil aliud est, quàm methodus à Mercatore usurpata, in ejus Logarithmotechnia, ad hyperbolam quadrandam, generalis reddita. D. Jacobus Gregorius apud Scotos superrimè incidit in eandem methodum.

N° XXII. *Ex epistola D. J. Collins in Italiam ad D. Alphonsum Borellum missa; & mense Decembri anno 1671 data: cujus habetur exemplar, manu ipsius D. Collins descriptum.*

Kinckhuysenii introductio ad Analysin Speciosam, quam *Stiel-konst* vocat, à D. Isaac Newtono prælo parata est; qui jam Mathematices Professor apud Cantabrigienfes factus est. Huic adjunget ipsius methodum generalem Quadraturarum analyticam; cujus ope calculo eruitur omnium curvilinearum figurarum regularium, communi aliquâ proprietate gaudentium, aream; earundem Curvarum rectificationem; inventionem centrorum gravitatis earum; itemque rotunda Solida, & superficies eorum rotatione genitas; & secunda istorum Solidorum segmenta: imò dato quovis logarithmico sinu, tangente vel secante in canone, invenire licet arcum ei competentem, absque naturali sinu, tangente vel secante prius invento, & vice versa; idque generaliter, sine ullâ radicum extractione.

Hujus specimen pro circulo apposui.

N. B. In hujus epistola exemplari, locus vacuus series interserenda hic relicta fuit.

N° XXIII. *Ex epistola ejusdem D. Collins ad D. Franciscum Vernon Anglum Parisiis tum agentem, Londini, 26 Decembris, anno 1671 data: cujus habetur exemplar manu ipsius D. Collins descriptum.*

D. Barrovius certior me facit, D. Newtonum pene adornasse Kinckhuysenii ad Algebram introductionem; cujus hic brevi edendæ negotium mihi curæ erit: eamque de propria ipsius penu auctiorem reddidisse. Huic subjiciet generalem * suam infinitarum serierum methodum analyticam; cujus ope computantur omnium spatiorum curvilinearum areæ, tum Geometricorum, tum eorum quæ, ex mente Cartesii, Mechanica sunt, modo figuræ una aliqua aut pluribus communibus proprietatibus definitæ sint; ipsarumque Curvarum longitudines; centra gravitatis; rotunda Solida, & superficies eorum rotatione genitæ. Hinc etiam eruntur multæ pro circulo series; necnon quovis numero dato, tanquam logarithmico sinu, tangente vel secante, calculo perfacili, sine ullâ radicum extractione, sine ullis tabulis, inveniri potest arcus ei respondens, & vice versa; idque vero quantum velis proximè, absque naturali sinu, tangente, aut secante prius invento: tot tantisque commodis facta est hæc doctrina, de quâ non nisi comperta loquor! Unâ cum his mittet viginti lectiones ejus Opticas, quas D. Barrovius opus censet, quo majus præsens ætas vix protulit. Admonui maturam

* Hic tractatus unus idemque est ac ille, cujus mentionem fecerat D. Newtonus in epistola 24. 1676, data, per D. Oldenburgum D. Leibaltio communicatâ; & in quo methodi serierum

randam ideo esse impressionem, quoniam D. Hugenius tractatum de Dioptrica, & de Curvarum Evolutione jam molitur. Ille autem contrâ, se magis cupere, ut accepto harum rerum nuncio, Hugenius potius excitaretur quàm tardaretur; ratus minimè verisimile utriusque hypotheses vel deductiones easdem esse posse.

Ex epistola D. J. Collins ad D. Thomam Strode, 26 Julii anno 1672 N° XXIV. data: cujus habetur exemplar manu ipsius D. Collins descriptum.

Quod geometriam curvarum figurarum spectat; hanc tandem generaliter ad calculum analyticum reduci posse, omnino orbi literato novum atque inauditum est. Hujus æquationes sunt series, terminis numero infinitis constatæ, quorum tamen pauci sufficiunt communiter, ex notis curvarum proprietatibus eruatæ. Auctorem quod attinet, hujusque methodi præstantiam, hæc accipe.

Mense Septembri 1668, Mercator Logarithmotechniam edidit suam, quæ specimen hujus methodi, i. e. serierum infinitarum, in unicâ tantum figurâ, nempe quadraturam hyperbolæ continet. Haud multo post quàm in publicum prodierat liber, exemplar ejus Cl. Wallisio Oxoniam misi; qui suum de eo judicium in Actis Philosophicis statim fecit: aliumque Barrovio Cantabrigiam; qui quasdam Newtoni chartas, qui jam Barrovium in mathematicis prælectionibus publicis excipit, extemplo remisit: è quibus & ex aliis, quæ olim ab auctore cum Barrovio communicata fuerant, patet illam methodum à dicto Newtono aliquot annis antea excogitatam, & modo universali applicatam fuisse: ita ut ejus ope, in quavis figurâ curvilinæa posita, quæ unâ vel pluribus proprietatibus definitur, quadratura vel area dictæ figuræ, + accurata, si possibile sit, sin minus infinite vero propinqua; evolutio, vel longitudo linearum curvæ; centrum gravitatis figuræ; Solida, ejus rotatione genita, & eorum superficies; sine ullâ radicum extractione obtineri queant.

Postquam intellexerat D. Gregorius hanc methodum, à D. Mercatore in Logarithmotechnia usurpatam, & Hyperbolæ quadrandæ adhibitam, quamque adauxerat ipse Gregorius, jam universalem redditam esse, omnibusque figuris applicatam; acri studio eandem acquisivit, multumque in eâ enodandâ desudavit.

Uterque, D. Newtonus & Gregorius, in animo habet hanc methodum adornare. D. Gregorius autem, D. Newtonum primum ejus inventorem anticipare, haud integrum ducit.

Ex epistola ad D. Collins ad D. Newtonum, 30 Julii anno 1672 data, cujus N° XXV. habetur exemplar manu ipsius D. Collins descriptum.

Parandis seriebus pro extrahendis radicibus in speciebus [Algebraicis], ad modum Vietæ [in numericis], credo D. Gregorium haud modicam inpen-

sierum infinitarum & fluxionum simul explicabantur, ut ibi loci memorat.

+ Testibus igitur Barrovio & Collinio, methodus prædicta quadrandi figuræ, accuratè si fieri possit, Newtono innotuit anno 1666 aut antea.

diffic

diffe ôperam: nihil autem de eâ re scribere fufcipiet, antequam tu, me-
rhodi hujus repertor, proprias de eâ lucubrationes in lucem emiseris; sed
aliis rebus per interim intentus est.

Nº XXVI. *Ex epistolâ D. Newtoni ad D. Collins, anno 1672, 10 Decembris datâ.*
Repertum autem est ipfius Newtoni autographum in fcriniis D. Collins, unâ
cum ejusdem exemplari manu D. Collins descripto.

Ex animo gaudeo D. Barrovii, amici nostri reverendi, lectiones Mathema-
ticis exteris adeo placuisse, neque parum me juvat intelligere eos [Slusium
& Gregorium] in eandem mecum incidisse ducendi tangentes methodum.
Qualem eam esse conjiciam, ex hoc exemplo percipies. Pone CB applica-
tam ad AB, in quovis angulo dato, terminari ad quamvis Curvam, AC; &

dicatur AB, x ; & BC, y ; habitudoque inter x & y exprimitur
quâlibet æquatione; puta $x^3 - 2xy + bxx - bbx + byy - y^3 = 0$, quâ ipsa determinatur Curva. Regula ducendi
tangentem hæc est: Multiplica æquationis terminos per
quamlibet progressionem arithmeticam juxta dimensiones
 y ; puta $x^3 - 2xy + bxx - bbx + byy - y^3$: ut & juxta dimensiones x ; puta

$x^3 - 2xy + bxx - bbx + byy - y^3$. Prius productum erit numerator, & pos-
terius, divisum per x , denominator fractionis, quæ exprimet longitudinem
AD, ad cujus extremitatem, D, ducenda est tangens CD. Est ergo longitudo
BD = $\frac{-2xy + 2byy - 3y^3}{3xx - 4xy + 2bx - bb}$.

Hoc est unum particulare, vel corollarium potius, methodi generalis;
quæ extendit se, citra molestum ullum calculum, non modo ad ducendum
tangentes ad quasvis Curvas, five Geometricas, five Mechanicas, vel quo-
modocunque rectas lineas, aliasve Curvas, respicientes; verum etiam ad re-
solvendum alia abstrusiora problematum genera de curvitatibus, areis, lon-
gitudinibus, centris gravitatis Curvarum, &c. Neque, quemadmodum
Huddenii methodus de Maximis & Minimis, ad solas restringitur æqua-
tiones illas, quæ quantitibus surdis sunt immunes.

Hanc methodum intertexui * alteri isti, quâ æquationum exegefin insti-
tuo, reducendo eas ad series infinitas. Memini me ex occasione aliquando
narrasse D. Barrovio, edendis lectionibus suis occupato, instructum me esse
hujusmodi methodo tangentes ducendi: sed nescio quo diverticulo, ab eâ
ipfi describendâ fuerim avocatus.

Slusii methodum tangentes ducendi brevi publicè prodituram confido:
quamprimum advenerit exemplar ejus, ad me transmittere ne grave ducas.

* Sc. in tractatu, quem Newtonus scripsit anno 1671. Missum autem fuit apographum hujus
epistolæ ad Tschurnhausium mense Maio 1675, & ad Leibnizium mense Junio 1676.

Epistola D. Slusii ad Oldenburgh, anno 1673, 17 Januarii Leodii
data, quâ continetur methodus ejus ducendi tangentes, inter epis-
tolas Regiæ Societatis asservatur Lib. Nº VI. pag. 11. Legitur
autem impressa in Transactionibus Philosophicis, Nº XC.

Ex epistolâ D. Oldenburgh ad D. Slusium, anno 1673, 29 Januarii datâ, Nº XXVII.
quâ prædictis Slusii literis respondetur. Legitur autem exemplar ejus in
libris Regiæ Societatis, Nº VI. p. 27.

Statui, Deo dante, primâ occasione methodum ipsam, prout epistolâ tuâ
continetur, Transactionibus Philosophicis inserere. Non ingratum interea
fuerit, accipere quæ doctissimus noster Newtonus, in Academiâ Cantabrigi-
ensi Mathematicum Professor, de eodem argumento ad D. Collinium nos-
trum, qui te summopere & jugiter colit, nuper perscripsit in hæc verba.

“ Non parum me juvat intelligere, mathematicos externos in eandem
“ mecum incidisse ducendi tangentes methodum. Qualem eam esse conj-
“ ciam, ex hoc exemplo percipies. *Aique ita deinceps ut in præcedente ip-*
“ *fius Newtoni epistolâ habetur.*”

Hactenus Newtonus, quæ ideo nunc perscribo, ut cum novissimis tuis
comparare possis.

Epistola D. Slusii ad D. Oldenburgh, anno 1673, 3 Maii Leodii data, quâ Nº XXVIII.
continentur fundamenta methodi tangentium Slusianæ, cujusque asservatur
exemplar in libris epistolarum Regiæ Societatis, Nº VI. p. 111, impressa le-
gitur in Phil. Transact. Nº XCV.

Ex epistolâ D. Oldenburgh ad D. Slusium, anno 1673, 10 Julii datâ. Le. Nº XXIX.
gitur autem inter epistolas Regiæ Societatis, Lib. VI. p. 196.

En tibi, vir illustrissime, impressum modum tuum demonstrandi me-
thodum tuam ducendi tangentes ad quaslibet Curvas; quemadmodum pos-
tremis tuis literis eum mihi communicaveras. Subticiui viri nomen offen-
sionis evitandæ causâ. Scripsit mihi D. Newtonus in hanc sententiam.

“ Ex priori tuâ epistolâ subdubitabam, existimaretne celeberrimus Slu-
“ sius per ea, quæ ipsi de me scripseras, me mihi tribuere methodum ip-
“ sius ducendi tangentes; donec intelligerem à D. Collinio, te ipsi signi-
“ ficasse, eam, ex opinione tuâ, seriùs hîc inventam fuisse. Tibi quippe
“ videtur, eam D. Slusio perspectam fuisse, aliquot annis priusquam ederet
“ Mesolabum suum, proindeque antequam ego eam intelligerem. At si
“ res secus se haberet, cum tamen eam primus communicaverit amicis suis
“ & literato orbi, jure merito ipsi debetur. Quoad methodos illas, cæ-
“ dem sunt, quanquam, crediderim, ex principis diversis derivatæ. Nesci
“ cio tamen num ipsius principia eam largiantur adeo generalem, ac mea
“ *quæ*

"quæ ad æquationes terminis furdus affectas se extendunt, absque eorum ad aliam formam reductione." Hæc ille, quæ in bonam partem à te acceptum iri confido.

Nº XXX. *Excerpta ex epistola D. Gothofredi Guilielmi Leibniti ad D. Oldenburgh, Londini, anno 1672, 3 Feb. datâ. Hujus autographon in scriptis Regiæ Societatis extat, & exemplar ejus in libro epistolarum dictæ Societatis, Nº VI. p. 35, descriptum legitur.*

Cùm heri apud illustrissimum Boylium, incidissem, in clarissimum Pellium, Mathematicum insignem, ac de numeris incidisset mentio; commemoravi ego, ductus occasione sermonum, esse mihi methodum ex quodam differentiarum genere, quas voco generatrices, colligendi terminos seriei cujuscunque continuè crescentis, vel decrescantis. Differentias autem generatrices voco, si datæ seriei inveniuntur differentia, & differentia differentiarum, & ipsarum ex differentiis differentiarum differentia, &c. & series constituatur ex termino primo & primâ differentia, & primâ differentia differentiarum, & primâ differentia ex differentiis differentiarum, &c. ea series erit differentiarum generatricium, ut si series continuè crescens vel decrescens fuit a, b, c, d , positâ Ω differentia notâ differentia generatrices erunt.

$$1a. 2a\Omega b. 3a\Omega b\Omega b\Omega c. 4a\Omega b\Omega b\Omega b\Omega c\Omega c\Omega d.$$

$$4. a\Omega b\Omega b\Omega c\Omega b\Omega c\Omega c\Omega d$$

$$3. a\Omega b\Omega b\Omega c\Omega b\Omega c\Omega c\Omega d$$

$$2. a\Omega b\Omega b\Omega c\Omega c\Omega d$$

$$1. a\Omega b\Omega c\Omega d$$

Aut in numeris; si series sit numerorum cubicorum deinceps ab unitate crescentium, differentia generatrices erunt numeri 0, 1, 6, 6. Voco autem generatrices, quia ex iis, certo modo multiplicatis, producuntur termini seriei; cujus usus tum maximè apparet, cùm differentia generatrices sunt finitæ, termini autem seriei infiniti; ut in proposito exemplo numerorum cubicorum.

$$\begin{array}{cccccccccccc} & & & 0 & & 0 & & 0 & & & & & \\ & & 6 & & 6 & & 6 & & 6 & & & & \\ 6 & & 12 & & 18 & & 24 & & 30 & & & & \\ 1 & & 7 & & 19 & & 37 & & 61 & & 91 & & \\ 0 & & 1 & & 8 & & 27 & & 64 & & 125 & & 216 \end{array}$$

Hoc cùm audisset clarissimus Pellius; respondit, id jam fuisse in literas relatum à D. Mouton Canonico Lugdunensi, ex observatione nobilissimi viri Francisci Regnaldi Lugdunensi, dudum in literario orbe celebris, in libro laudati D. Mouton de diametris apparentibus Solis & Lunæ. Ego, qui ex epistola quâdam à Reginaldo ad Monconium scriptâ, & diario itinerum Monconiano insertâ, nomen D. Moutoni & designata ejus duo didiceram; diametros luminarium apparentes, & consilium de mensuris rerum ad posteros transmittendis; ignorabam tamen librum ipsum prodixisse. Quare apud D. Oldenburgium Societatis Regalis Secretarium, sumtum mutuo tum-

multuariè percurri, & inveni verissimè dixisse Pellium. Sed & mihi tamē dandam operam credidi, ne qua in animis relinqueretur suspicio, quasi, tacito inventoris nomine, alienis meditationibus honorem mihi quærere, voluissem; & spero apparitum esse, non adeo egenum me meditationum propriarum, ut cogar alienas emendicare. Duobus autem argumentis ingenuitatem meam vindicabo. Primò si ipsas schedas meas confusas, in quibus non tantum inventio mea, sed & inveniendi modus occasioque apparet, monstrarem: deinde si quædam momenti maximi, Reginaldo Moutonoque indicta, addam, quæ ab hesterno vespere confinxisse me non sit verisimile, quæque non possunt facile expectari à transcriptore.

Ex schedis meis occasio inventi hæc apparet. Quærebam modum inveniendi differentias omnis generis potestatum; quemadmodum constat differentias quadratorum esse numeros impares: inveneramque regulam generalem ejusmodi.

Datâ potentia gradûs dati præcedente, invenire sequentem (vel contrâ) distantia datæ, vel radicum datarum; seu invenire potentiarum gradûs dati, utcunque distantium, differentias. Multiplicetur potentia, gradûs proximè præcedentis, radices majoris per differentiam radicem; & differentia potentiarum, gradûs proximè præcedentis, multiplicetur per radicem minorem. Productorum summa erit quæsitâ differentia potentiarum, quarum radices sunt datæ. Eandem regulam ita inflexeram, ut sufficeret, præter radices cujuscunque gradûs, etiam non proximè præcedentis, potentias datarum radicum dari, ad differentias potentiarum alterius cujuscunque, licet altioris, gradûs inveniendas. Et ostendi quod in quadratis observatur, numeros impares esse eorum differentias, id non nisi regulæ propositæ subsumptionem esse.

His meditationibus defixus, quemadmodum in quadratis differentia sunt numeri impares, ita quoque quæsi, quales essent differentia cuborum; quæ cùm irregulares viderentur, quæsi differentias differentiarum, donec inveni differentias tertias esse numeros senarios. Hæc observatio mihi aliam peperit. Videbam enim ex differentiis præcedentibus generari terminos differentiasque sequentes; ac proinde, ex primis, quas ideo voco generatrices, ut hoc loco 0.1.6.6, sequentes omnes. Hoc concluso, restabat invenire, quo additionis, multiplicationisve, aut horum complicationis genere, termini sequentes ex differentiis generatricibus producerentur. Atque ita resolvendo experiundoque deprehendi primum terminum, 0, componi ex primâ differentia generatrice, 0, sumptâ semel, seu, vice unâ: secundum, 1, ex primâ 0 semel, & secundâ 1 semel: tertium, 8, ex primâ 0 semel, secundâ 1 bis, & tertiâ 6 semel: nam $0 \times 1 + 1 \times 2 + 6 \times 1 = 8$. Quartum, 27, ex primâ 0 semel, secundâ 1 ter, tertiâ 6 ter, quartâ 6 semel: nam $0 \times 1 + 1 \times 3 + 6 \times 3 + 6 \times 1 = 27$, &c. idque analysis mihi universale esse comprobavit. Hæc fuit occasio observationis meæ, longè alia à Moutonianâ; qui cùm in tabulis condendis laboraret, in hoc calculandi compendium cum Reginaldo incidit. Nec vel illi vel Reginaldo adimenda laus, quod & Briggs in logarithmicis suis jam olim talia quædam, observante Pellio, ex parte advertit. Mihi hoc superest; ut addam nonnulla illis indicta, ad amolendum

liendum transcriptoris nomen; neque enim interest reipublicæ, quis observaverit; interest, quid observetur. Primum ergo illud adjicio, quod apud Moutonium non extat, & caput tamen rei est: quinam sint illi numeri, quorum tabulam ille exhibet in infinitum continuandam, quorum ductu in differentias generatrices, productis inter se junctis, termini serierum generentur. Vides enim ex ipso modo, quo tabula ab eo pag. 385 exhibetur, non fuisse id ei satis exploratum; alioqui enim verisimile est ita tabulam fuisse dispositurum, ut ea numerorum connexio atque harmonia appareret; nisi quis de industriâ texisse dicat: ita enim se habet pars tabulæ.

1	1					
2	1	1				
3	1	2	1			
(4)	1	3	3	1		
5	1	4	6	4	1	
6	1	5	10	10	5	1
7	1	6	15	20	15	6
8	1	7	21	35	35	21
9	1	8	28	56	70	56
10	1	9	36	84	126	126
11	1	10	45	120	210	252

Apparet, ex hujus tabulæ constructione, solam haberi rationem correspondens numerorum generantium cum numero termini generati; ut cum terminus est quartus (4) producitur ex primâ differentiâ semel, secundâ ter 3, tertiâ ter 3, quartâ semel 1; ideo in eadem (4) lineâ transversâ locantur 1.3.3.1. Sed vel non observavit, vel dissimulavit, autor correspondens numerorum, si à summo deorsum eundo per columnas disponantur hoc modo,

1	1					
2	1	< 1				
3	1	2	< 1			
4	1	3	3	< 1		
5	1	4	6	4	< 1	
6	1	5	10	10	5	< 1
7	1	6	15	20	15	6
8	1	7	21	35	35	21
9	1	8	28	56	70	56
10	1	9	36	84	126	126
11	1	10	45	120	210	252

Ita enim statim vera genuinaque eorum natura ac generatio apparet; esse scilicet eos numeros, quos combinatorios appellare soleo, de quibus multa dixi in dissertatiunculâ de arte combinatoriâ; quosque alii appellant ordines numericos; alii in specie primam columnam, unitatem; secundam, numerorum naturalium; tertiam, triangularium; quartam, pyramidalium; quintam, triangulo triangularium &c. de quibus integer extat tractatus Paschali sub titulo Trianguli Arithmetici; in quo tamen proprietatem numerorum

eorum ejusmodi tam illustrem tamque naturalem * non observatam sum miratus. Sed est profecto casus quidam in inveniendis, qui non semper maximis ingeniis maxima, sed sæpe etiam mediocribus nonnulla offert.

Hinc jam vera numerorum istorum natura, & tabulæ constructio, sive à Reginaldo sive à Moutonio dissimulata, intelligitur: semper enim terminus datæ columnæ datæ componitur ex termino præcedente columnæ tam præcedentis quàm datæ: atque illud quoque apparet, non opus esse molesto calculo ad tabulam, à Moutonio propositam, continuandam, ut ipse postulat; cum hæ numerorum series passim jam tradantur calculenturque.

Cæterum Moutonius observatione istâ, ad interponendas medias proportionales inter duos extremos numeros datos; ego, ad inveniendos ipsos numeros extremos in infinitum, cum eorum differentiis, utendum censebam. Hinc ille, non nisi cum differentiæ ultimæ evanescunt, aut pene evanescunt, usum regulæ invenit; ego detexi innumerabiles casus, regulâ quâdam inobservatâ comprehendendos, ubi possum ex datis numeris finitis, certo modo multiplicatis, producere numeros plurimarum serierum in infinitum euntium, etsi differentiæ earum non evanescant.

Ex iisdem fundamentis possum efficere in progressionibus problemata plurima; aut in numeris singularibus, aut in rationibus vel fractionibus. Possum enim progressionem addere subtrahereque, imo multiplicare quoque & dividere, idque compendiosè.

$\frac{1}{3}$	\cdot	$\frac{1}{4}$	\cdot	$\frac{1}{5}$	\cdot	$\frac{1}{6}$
$\frac{1}{6}$	\cdot	$\frac{1}{10}$	\cdot	$\frac{1}{15}$	\cdot	$\frac{1}{21}$
$\frac{1}{10}$	\cdot	$\frac{1}{20}$	\cdot	$\frac{1}{35}$	\cdot	$\frac{1}{56}$
$\frac{1}{15}$	\cdot	$\frac{1}{35}$	\cdot	$\frac{1}{70}$	\cdot	$\frac{1}{126}$
&c.		&c.		&c.		&c.

Multa alia circa hos numeros observata sunt à me, ex quibus illud eminet; quòd modum habeo summam inveniendi seriei fractionum in infinitum decrecentium; quarum numerator unitas, nominatores verò numeri isti triangulares aut pyramidales, aut triangulo-triangulares, &c.

* Imò observata fuit. Vide Paschali Triangulum Arithmeticum, Parisiis anno 1665 editum, pag. 2, ubi definitionum antepenultima hæc est.

Le nombre de chaque cellule est egal a celuy de la cellule qui la precede, dans son rang perpendiculaire, plus a celuy de la cellule qui la precede dans son rang parallele. Ainsi la cellule F, c'est à dire le nombre de la cellule E, egale la cellule c plus la cellule E; & ainsi des autres.

N° XXXI. *In scriptis etiam Regiæ Societatis asservantur autographa quinque epistolarum, à D. Leibnitio ad D. Oldenburgum eodem anno 1673 scriptarum; prima autem Londini data est, Februarii 20; reliquæ vero Parisiis, Martii 30, Aprilis 26, Maii 24, & Junii 8. Omniumque, si secundum excipias, exemplaria leguntur in libro Regiæ Societatis N° VI. pag. 34, 101, 115 & 137.*

N° XXXII. *Hactenus D. Leibnitius in Arithmetica versabatur: jam ad Geometriam se convertit; & anno proximo ad Oldenburgum scribit epistolas duas, Parisiis Jul. 15, & Octob. 36 datas, quæ leguntur in libro epistolarum Regiæ Societatis N° VII. pag. 93 & 110, eademque reperiuntur impressæ in tomo tertio Operum Mathematicorum D. J. Wallis, & in scriptis Reg. Societatis asservantur earum autographa.*

Ex harum priore 15 Julii datâ.

Diu est, quod nullas à me habuisti literas, &c. Alia mihi theoremata sunt, momenti non paulò majoris. Ex quibus illud imprimis mirabile est, cujus ope area circuli, vel sectoris ejus dati, exactè exprimi potest per seriem quandam numerorum rationalium continuè productam in infinitum. Sed & methodos quasdam analyticas habeo, generales admodum & latè fufas, quas majoris facio quàm theoremata particularia & exquisita.

Ex posteriore 26 Octob. datâ.

N° XXXIII. Porro, in eâ geometriæ parte rem memorabilem mihi evenisse nuncio. Scis D. Vicecomitem Brounkerum, & Cl. Nic. Mercatorem exhibuisse infinitam seriem numerorum rationalium, spatio Hyperbolæ æqualem. Sed hoc in Circulo efficere, hæcenus potuit * nemo. Etti enim illi Brounkerus & Wallisius dederint numeros rationales magis magisque appropinquantes; nemo tamen dedit [*imo uterque dedit; sed forte non ejus sensu*], progressionem numerorum rationalium, cujus, in infinitum continuatæ, summa sit exactè æqualis circulo. Sed verò mihi tandem feliciter successit. Inveni enim seriem numerorum valdè simplicem, cujus summa exactè æquatur circumferentiæ circuli; posito diametrum esse unitatem. Et habet ea series id quoque peculiare, quod miras quasdam circuli & hyperbolæ exhibit harmonias. Itaque tetragonismi circularis problema, jam à Geometria traductum est ad Arithmetica Infinitorum: quod hæcenus frustra quærebatur. Restat ergo tantum, ut doctrina de serierum, seu progressionum numericarum, summis perficiatur. Quicumque hæcenus quadraturam circuli exactam quæsiere, ne viam quidem aperuere, per quam eò pervenire possit spes sit: *quid nunc primum à me factum dicere ausim.* Ratio diametri ad circumferentiam exactè à me exhiberi potest per rationem, non quidem numeri ad numerum, id enim foret absolute invenisse; sed per rationem numeri

* Collinius jam ante quadriennium series Newtonianas, ante triennium Gregorianas, cum amicis communicare cepit. Leibnitius in Angliâ diversabatur anno superiore (1673), & hujusmodi series nonnumquam communicaverat; nec prius cum amicis in Angliâ communicare cepit, quàm ab

numeri ad totam quandam seriem numerorum rationalium valdè simplicem & regularem. Eadem + methodo etiam arcus cujuslibet, cujus sinus datur, geometricè exhiberi, per ejusmodi seriem, valor potest; nullo ad integræ circumferentiæ dimensionem recursum. Ut adeo necesse non sit, arcus rationem ad circumferentiam nosse.

Excerpta ex epistolâ D. Oldenburg ad D. Leibnitium, anno 1674, 8 Dec. N° XXXIV. cembriis datâ, cujus asservatur autographum. Eadem autem legitur inter epistolas Regiæ Societatis, lib. N° VII. pag. 119; estque responsum ad litteras D. Leibnitii, 26 Octobris præcedentis datas.

Quod de profectu in curvilinearum dimensione memoras, bene se habet; sed ignorare te nolim Curvarum dimetiendarum rationem & methodum à laudato Gregorio, nec non ab Isaaco Newtono, ad Curvas quaslibet, tum mechanicas, tum geometricas, quin & circulum ipsum, se extendere: ita scilicet ut si in aliquâ Curvâ ordinatam dederis, istius methodi beneficio possis lineæ curvæ longitudinem, aream figuræ, ejusdem centrum gravitatis, Solidum rotundum, ejusque superficiem, sive erectam sive inclinatam, Solidique rotundi segmenta secunda, horumque omnium conversâ invenire: quin & dato quolibet arcu in quadrante, logarithmicum sinum, tangentem vel secantem, non cognito naturali, & conversum computare. Quod verò ais neminem hæcenus dedisse progressionem numerorum rationalium, cujus, in infinitum continuatæ, summa sit æqualis circulo; id verò tibi tandem feliciter successisse, de eo quidem tibi gratulor, &c.

Ex epistolâ D. Leibnitii ad D. Oldenburg, Parisiis anno 1675, 30 Martii N° XXXV. datâ. Exstat autographum scriptoris; & reperitur descripta inter epistolas Reg. Soc. N° VII. pag. 213. Hæc autem respondetur ad supradictas Oldenburgi litteras, 8 Decembris præcedentis datas.

Scribis, clarissimum Newtonum vestrum habere methodum exhibendi quadraturas omnes, omniumque curvarum superficierum, & Solidorum, ex revolutione genitorum, dimensiones, & centrorum gravitatis inventiones; per appropinquationes scilicet, ita enim interpretor. Quæ methodus, si est universalis & commodâ, meretur æstimari; nec dubito fore ingeniosissimum authore dignam. Addis, tale quid Gregorio innotuisse.

Ex epistolâ D. Oldenburg ad D. Leibnitium, anno 1675, 15 Aprilis datâ; N° XXXVI. cujus habetur exemplar inter epistolas Reg. Societatis N° VII. pag. 216. Hæc respondetur ad D. Leibnitii litteras, 30 Martii præcedentis datas: Anglicè autem extat, manu D. Collins designata ac 10 Aprilis data, eamque Latine transtulit D. Oldenburg & ad D. Leibnitium misit.

D. Collinius, præmissâ salute, quæ sequuntur remittit. Primo Cl. Gre-

ab Oldenburg accepit, ut mox patebit; neque alias communicavit, quàm quas acceperat.

+ Methodum exhibendi arcum cujus sinus datur, Leibnitius ab Oldenburg postea quæsit, Maii 12, 1676.

gorium, in postremâ suâ ad illustrem Hugenum responsione, seriem suppetitâsse ad semicircumferentiam circuli inveniendam, quæ talis.

Pone radium = r ; dimidium latus quadrati inscripti circulo = d ; & differentiam inter radium & latus quadrati = e : semicircumferentia æqualis est $\frac{4rr}{2d - \frac{e}{3} - \frac{e^3}{90d} - \frac{e^5}{756d^3} - \frac{e^7}{113400d^5} - \frac{e^9}{7484400d^7} - \&c.}$ in infinitum; quæ

series adeo produci potest, ut à semicircumferentiâ minus differat, quam ulla quantitas assignabilis.

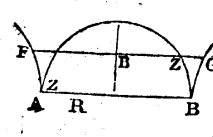
Editum hoc fuit à D. Gregorio, postquam D. Mercatoris Logarithmotechnia jam extabat; quæ, quàm primùm viderat lucem, ad D. Barrovium à me fuit transmissa; qui, observato in eâ infinitæ seriei usu ad logarithmos construendos, rescribat, methodum illam jam aliquandiu excogitatam fuisse à successore suo Newtono; omnibusque Curvis, earumque portionibus, geometricis æquæ ac mechanicis universim, applicatam: cujus rei specimina quædam subjecit, viz.

Positâ pro radio unitate, datoque x pro sinu, ad inveniendum z arcum series hæc est: $z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{16128}x^9 + \&c.}$ in infinitum. Et extractâ radice hujus æquationis, methodo symbolicâ, si dederis z pro arcu, ad inveniendum x sinum series hæc est;

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9 - \&c.$$

Atque hæc series facillè continuatur in infinitum. Prioris beneficio, ex sinu 30 grad. Ceulenii numeri facillè struuntur.

Consimiliter si ponas radium R , & B sinum arcûs; zona, inter diametrum & chordam illi parallelam, est



$$= 2RB - \frac{B^3}{3R} - \frac{B^5}{20R^3} - \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} - \frac{7B^{11}}{1408R^9} - \&c. \text{ Atque eadem series, mutatis signis termini secundi, quarti \& sexti, \&c. inservit assignandæ areæ zonæ æquilateralis hyperbolæ, viz.}$$

$$AFGB = 2RB + \frac{B^3}{3R} - \frac{B^5}{20R^3} + \frac{B^7}{56R^5} - \frac{5B^9}{576R^7} + \frac{7B^{11}}{1408R^9} - \&c.$$

Rursum, dato radio R , & sinu versio sive fagittâ a , ad inveniendam aream segmenti resecti à chordâ: pone b^* pro $2Ra$, & erit segmentum $= \frac{4ba}{3} - \frac{2a^3}{5b} - \frac{a^5}{14b^3} - \frac{a^7}{36b^5} - \frac{5a^9}{352b^7} - \frac{7a^{11}}{832b^9} - \&c.}$

$$\text{Et arcus integer} = 2b + \frac{a^2}{4b} + \frac{3a^4}{20b^3} + \frac{5a^6}{56b^5} + \frac{35a^8}{576b^7} + \frac{63a^{10}}{1408b^9} + \&c.$$

Dux hæc series D. Gregorio habentur; quas exhibuit ex eo tempore, quo usus est hæc methodo; quod ab ipso aliquot post annis factum: postquam scilicet intellexerat D. Newtonum generatim eam applicâsse. Exinde quoque ad nos misit series similes ad tangentes naturales ex earundem arcubus,

* Hanc seriem D. Collins, initio anni 1671, à Gregorio acceperat, ut supra; D. Leibnitius eandem cum amicis in Galliâ hoc anno ut suam communicavit, celatâ hæc epistolâ.

† His verbis patet series, quas D. Leibnitius se ante annos aliquot invenisse professus est, à communicatis diversas fuisse. Subjungit etiam ipse verbis disertis sua à communicatis longe diversa esse circa hanc rem meditata. Vid. epist. Maii 12, 1676.

& conversim, obtinendum. Ex. gr. pone radium = r ; arcum, a ; tangentem, t : erit, $t = a + \frac{a^3}{3r^2} + \frac{2a^5}{15r^4} + \frac{17a^7}{315r^6} + \frac{62a^9}{2835r^8} + \&c.}$ Et conversim ex tangente invenire arcum ejus, $* a = t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} + \frac{t^9}{9r^8} - \&c.}$

Atque hoc factum cum vides, facillè credideris, posse eadem methodo æquæ facillè ex arcu inveniri sinum vel tangentem logarithmicum, absque inventionem naturalis, & conversim. Pronum quoque tibi fuerit credere, methodum hanc applicari posse ad rectificationem quarumlibet Curvarum; particulatim verò ad lineam quadratricem, & ad inveniendam aream illius figuræ: id quod antehac, nullâ demum cum methodo, fuit præstitum. Atque ulteriore calculationis labore extendi potest ad inveniendas areas superficierum in rotundis Solidis inclinantibus; nec non ad inveniendas Soliditates secundorum segmentorum in Solidis rotundis. E. G. Si Conoides aliqua secetur à plano transeunte per basin ejus, poterit id vocari segmentum primum; & si hæc portio iterum secetur à plano, recto ad planum prius secans, portio, eum in modum secta, hoc ipso intenditur, ut sit segmentum [secundum].

Porro methodus eadem applicatur inveniendis radicibus purarum potestatum, valde affectarum æquationum; ita ut ex quolibet numero, absque logarithmorum ope, excitare possis quamlibet potestatem per saltum, & ex quâvis potestate, utut affectâ, invenire radicem ejus, vel quodvis medium illud inter & unitatem assignatum. D. Gregorius magno labore paravit seriem infinitam, generatim respectivis potestatibus affectis cujuslibet æquationis propositæ adaptandam; ita ut quivis Algebrae cultor, penu ipsius instructus, mox aptare possit seriem aliquam, ad inveniendam quamlibet radicem cujusvis æquationis propositæ; postquam innovit, ad quod latus noti limitis radix ceciderit. Verum id hætenus nobis non communicavit, uti nec nos illum ad id faciendum sollicitavimus; imprimis cum ipse lubens permittat Newtono, ut ille primus novæ hujus methodi de infinitâ serie inventionem orbi mathematico patefaciat, &c.

Ex epistolâ D. Leibnitii ad D. Oldenburgh, anno 1675, 20 Maii, Parisiis N° XXXVII. datâ. Extat autographon ejus, eademque legitur in libro epistolarum Regiæ Societatis, N° VII. pag. 235. Responsum autem est ad prædictas D. Oldenburghi literas 15 Aprilis datas.

Literas tuas, multâ fruge algebraicâ refertas, accepi; pro quibus tibi, & doctissimo Collinio, gratias ago. Cum nunc præter ordinarias curas mechanicis imprimis negotiis distrahar, non potui examinare series, quas misistis, ac cum tui meis comparare. Ubi fecero, † perferibam tibi sententiam meam: nam aliquot jam anni sunt, quod inveni meas, viâ quâdam sic satis singulari. Collinium ipsum magni facio; quoniam omnes puræ matheseos

† N. B. Hoc nunquam fecit D. Leibnitius; sed ubi series duas primas per Mohrum quendam denique accepisset, postulat methodum D. Newtoni, perveniendi ad istas duas series, ad se mitti; quas nullas prius à Oldenburgo accepisset. Et hoc pecto, epistolam Oldenburghi oblivioni tradendo, licentiam obtinuit serierum ab eo acceptarum ultimam tibi vindicandi.

partes ab ipso egregiè cultas video. Multa habeo destinata, à quibus me deterrent calculi tantum; qui nec suscipi facilè ab homine occupato, nec alteri, nisi doctissimo ac sincerissimo, tutò credi possunt.

Nº
XXXVIII.

Ex Aëtis Eruditorum anno 1691 mense Aprili, pag. 178, habentur hæc D. Leibnitii verba.

*Jam anno 1675 compositum habebam * opusculum Quadraturæ Arithmeticæ, ab amicis ab illo tempore lectum; sed quod, materiâ sub manibus crescente, illi mare ad editionem non vacavit, postquam aliæ occupationes supervenire; præsertim cum nunc prolixius exponere vulgari more, quæ Analysis nostra nova paucis exhibet, non satis operæ pretium videatur. Interim insignes quidam Mathematici, quibus veritas primariæ nostræ propositionis dudum in aëtis publicatæ innotuit, pro humanitate suâ, nostri qualiscunque inventi candidè meminere.*

Nº XXXIX. *Excerpta ex schediasmatis manu D. Collins exaratis, & in scriiniis ejus reperiis, & nonnullis in locis Oldenburgi calamo castigatis; quæ quidem D. Oldenburg D. Tschurnhausio transmittenda acceperat, & Latine verterat. Extant autem tum autographa D. Collins, tum responsum ad eadem D. Oldenburg reddidit, cum titulo manu ejus inscripto, "Responsum ad Scriptum D. Collinii de Cartesii Inventis, Accep. d. 8 Junii 1765."*

Nonnulli Cartesium arrogantiae infimulârunt, afferentem se ex omnibus modis methodivæ possibilibus, in optimam simplicissimamque incidisse. An ullibi hoc affirmaverit Cartesius plane nescio: certum tamen, est methodum ducendi tangentis multum promotam fuisse à Newtono & Gregorio. Ita liquet ex Newtoni epistolâ anno 1672, 10 Decemb. datâ. Vide Nº XXVI.

N. B. In hoc schediasmate habetur apographum epistolæ hujus, ut & apographum epistolæ Gregorii ad Collins 5 Sept. 1670, supra impressæ.

Nº XL.

Ex epistolâ D. Oldenburg ad D. Leibnitium, anno 1675, 24 Junii datâ, & in libro epistolarum Regiæ Societatis, Nº VII. pag. 243 descriptâ. Responsum autem est ad præcedentes D. Leibnitii literas, 20 Maii datas.

Dominus Newtonus, beneficio logarithmorum graduatorum in scalis, *παραλλήλων* locandis ad distantias æquales; vel circulorum concentricorum eo modo graduatorum adminiculo, invenit radices æquationum. Tres regulæ rem faciunt pro cubicis, quatuor pro biquadraticis. In harum dispositione respectivæ coefficientes omnes jacent in eadem lineâ rectâ; à cujus puncto, tam remoto à primâ regulâ ac scilicet graduatæ sunt ab invicem, lineâ rectâ iis superextenditur, unâ cum præscriptis conformibus genio æquationis; quâ in regularum unâ datur potestas pura radices quæsitæ.

Ex

* Quadratura Arithmetica, de qua hic agitur, ea est, quam Gregorius cum D. Collinio, initio anni 1671, Oldenburgus cum D. Leibnitio, hoc anno, communicavit. De hac quadraturâ D. Leibnitius opusculum vulgari more composuit, & cum amicis hoc anno communicare cepit: anno proximo scriptum polivit, ut cum Oldenburg committeretur: anno tertio in patriam redux negotiis

Ex epistolâ D. Leibnitii ad D. Oldenburg, Parisiis anno 1675, 12 Julii datâ. Nº XLI.

Hujus extat autographum; habeturque exemplar ejus in libro epistolarum Regiæ Societatis, Nº VII. pag. 149. Responsum autem est ad literas præcedentes D. Oldenburgi, & impressa legitur inter Opera D. Wallisii. In hac perperam scribitur Parisius pro Darius.

Methodum celeberrimi Newtoni, radices æquationum inveniendi per instrumentum, credo differre à meâ. Neque enim video in meâ, quid aut logarithmi aut circuli concentrici conferant. Quoniam tamen rem vobis non ingratam video, conabor absolvere, ac tibi communicare, quamprimum otii fat erit.

Scriptisti aliquoties, vestrates omnium Curvarum dimensiones per approximationem dare. Velim nosse, an possint dare geometricè dimensionem Curvæ ellipticos vel hyperbolæ, ex datâ circuli, aut hyperbolæ, quadraturâ.

Ex epistolâ D. Oldenburgi ad D. Leibnitium, anno 1675, 30 Septemb. Nº XLII. datâ. Cujus extat exemplar manu D. Oldenburg descriptum. Legitur etiam in libro Regiæ Societatis Nº VII. pag. 159, & responsum est ad præcedentem.

Scriptum quoddam Belgicum Belga quidam, Georgius Mohr vocatus, Algebrae & Mechanicæ probe peritus, apud Collinium nostrum reliquit; qui apographum ejus, quale hic insertum vides, impertire tibi voluit. Tschurnhausius nuper Parisios hinc profectus est; & te sine dubio jam salutavit. Scire cupis, an dare nostrates geometricè possint dimensionem Curvæ ellipticos aut hyperbolæ, ex datâ circuli, aut hyperbolæ, quadraturâ. Ait Collinius, illos id præstare non posse geometricâ præcisione; sed dare eos posse ejusmodi approximationes, quæ quâcunque quantitate datâ minùs à scopo aberrabunt. Et speciatim, quod attinet alicujus arcûs circuli rectificationem, impertire tibi poterit laudatus Tschurnhausius methodum, à Gregorio nostro inventam, quam, cum apud nos esset, Collinius ipsi communicavit.

Ex epistolâ D. Leibnitii ad D. Oldenburg, Parisiis 28 Decembris anno 1675 Nº XLIII. datâ. Extat autographum ejus, describiturque in libro Regiæ Societatis, Nº VII. pag. 189, & à D. Wallisio impressa est.

Duarum tibi literarum debitor, rogo ne sequius interpreteris silentium meum; soleo enim interrumpi nonnunquam, & hæc studia per intervalla tractare. Quod Tschurnhausium ad nos misisti, fecisti pro amico: multum enim ejus consuetudine delector; & ingenium agnosco in juvene præclarum & magna promittens. Inventi mihi ostendit non pauca, Analytica & Geometrica, sane perelegantia. Unde facilè judico, quid ab eo expectari possit.

gotiis publicis interesse cepit; & materiâ sub manibus crescente, opus ad editionem limare non amplius vacavit. Sed neque operæ pretium duxit, subinde prolixius exponere, vulgari more, quæ analysis sua nova paucis exhibet. Inventi est igitur hæc analysis, postquam D. Leibnitius opusculum, vulgari more compositum, polire & limare desit, & negotiis publicis interesse cepit.

VOL. IV.

X x x

Habebis

Habebis & à me instrumentum æquationes omnes geometricas construendi unicum : et meam quadraturam circuli, ejusque partium, per seriem numerorum rationalium infinitam ; de quâ aliquoties scripsi, & quam jam plufquam biennio abhinc geometris hîc communicavi.

N° XLIV. *Ex epistolâ D. Leibnitii ad D. Oldenburg, Parisiis, 12 Maii, anno 1676, datâ; cujus autographum in scriniis Regiæ Societatis asservatur, cum notis manu Oldenburgi in tergo scriptis.*

Cùm Gregorius Mohr Danus [*superius Belga*], in Geometrâ & Analyfi versatissimus, nobis attulerit communicatam sibi à doctissimo Collinio vestro expressionem relationis inter arcum & sinum, per infinitas series sequentes : posito sinu x , arcu z , radio 1.

$$z = x + \frac{1}{6}x^3 + \frac{3}{40}x^5 + \frac{5}{112}x^7 + \frac{35}{1152}x^9 \text{ \&c.}$$

$$x = z - \frac{1}{6}z^3 + \frac{1}{120}z^5 - \frac{1}{5040}z^7 + \frac{1}{362880}z^9 \text{ \&c.}$$

Hæc * INQUAM, cum nobis attulerit ille, quæ mihi valdè ingeniosa videntur ; & posterior imprimis series elegantiam quandam singularem habeat ; ideo rem gratam mihi feceris, vir clarissime, si demonstrationem transmiseris. Habebis vicissim mea ab his longè diversa, circa hanc rem meditata ; de quibus jam aliquotab hinc annis ad te perscripsisse credo ; demonstratione tamen non additâ, quam + nunc polio. Oro ut Cl. Collinio multam à me salutem dicas : is facile tibi materiam suppeditabit satisfaciendi desiderio meo.

N° XLV. *Ex epistolâ D. Collins ad D. Oldenburgum, D. Leibnitium tum Parisiis agenti transmittendâ. Hujus exemplar, anno 1676, 14 Junii, manu ipsius D. Collins descriptum, ac in scriniis ejus repertum, etiamnum conservatum est.*

Respondeas, si placet, ad ea quæ quærit D. Leibnitius, in literis ejus 12 Maii datis, seriei primæ numeros coefficientes, $\frac{1}{6}, \frac{3}{40}, \frac{5}{112}, \frac{35}{1152}$, hoc modo

compositos esse, $\frac{1 \times 1}{2 \times 3} = \frac{1}{6}$; & $\frac{1}{6} \times \frac{3 \times 3}{4 \times 5} = \frac{3}{40}$; & $\frac{3}{40} \times \frac{5 \times 5}{6 \times 7} = \frac{5}{112}$; & $\frac{5}{112} \times \frac{7 \times 7}{8 \times 9} = \frac{35}{1152}$; & $\frac{35}{1152} \times \frac{9 \times 9}{10 \times 11} = \frac{63}{2816}$; atque ita deinceps in infinitum. Unde intelligi possit, hanc seriem elegantiam minimè cedere conversæ ejusdem ; quæ tamen illi magis aridet. Meditata ejus de eodem argumento, cùm fundamentis planè diversis innitantur, non possunt nobis non esse acceptissima ; atque exoptamus ea fidem nostram exuperare posse. Hujus autem methodi ea est præstantia, ut cùm tam latè pateat, ad nullam hæreat difficultatem. Gregorium autem aliosque in eâ fuisse opinione arbitror, ut quicquid uspiam antea de hac re innotuit, quasi dubia diluculi lux fuit, si cum meridianâ claritate conferatur.

* Quasi ante annum eisdem non accepisset ab Oldenburg.

Hoc

Hoc anno, cùm D. Gregorius, anno superiore ad finem vergente, emortuus N° XLVI. esset, quæ cum amicis communicaverat in unum corpus, sollicitante D. Leibnitio, collecta sunt. Et exstat collectio, manu D. J. Collins exarata, cum hoc titulo ;

Excerpta ex D. Gregorii epistolis cum D. Leibnitio communicanda ; tibi quæ postquam perlegerit ille, reddenda. Et sic orditur.

D. H. Oldenburg Armigero.

Quandoquidem impensè rogasti me, permotus sollicitationibus D. Leibnitii & aliorum ex Academiâ Regiâ Parisinâ, ut historiolum aliquam concinnarem, studia & inventa doctissimi D. Jacobi Gregorii, nuper defuncti, exhibentem ; quoniamque arcta inter nos amicitia, crebraque dum viveret literarum reciprocatio fuit ; in honorem nominis ejus, quæcunque majoris momenti in literis ejus occurrunt, summâ fide in unum colligere statuo, &c.

In hac collectione habetur epistola superius impressa Gregorii ad Collins 5 Sept. 1670. Habetur & epistola superius impressa, quâ Gregorius quadraturam prædictam arithmetice, initio anni 1671, cum D. Collins communicavit. Habetur & epistola D. Newtoni ad D. Collins, 10 Decemb. 1672 data, & superius impressa ; in quâ Newtonus se methodum generalem habere dicit ducendi tangentes, quadrandi curvilineas, & similia peragendi ; & methodum exemplo ducendi tangentes exponit : quam methodum D. Leibnitius differentialem postea vocavit. Hæc collectio ad D. Leibnitium missa fuit 26 Junii 1676.

Ex epistolâ D. Collins ad D. Davidem Gregorium, prædicti Jacobi Gregorii N° XLVII. nuper defuncti fratrem. Data autem est anno 1676, 11 Augusti, ejusque habetur exemplar manu ipsius D. Collins descriptum.

Historiolum composui, quâ in unum congeffi, quæcunque unquam à fratre tuo, de rebus mathematicis, vel literis, aliasve scripto vel colloquio, acceperim : eo fine, ut eandem scriniis Regiæ Societatis (cujus erat sodalis) commissam & asservatam, amici ejus inspicere possint ; vel si libuerit, soluto pretio, transcriptam habere. Constat autem duodecim circiter schedis. Me verò nihil omisisse, quod alicujus momenti esse poterit, si nonnulla cum Hugenio aliisve controversa excipias, aras sacras juraturus contingere ausim. Mathematicis Gallis quousque profecerat, quæque reliquerat contingere ausim. Me operam dedisse ut iis satisfacerem, ex sequentibus comperies. Sub finem autem exempli hujus epistolæ hæc subiunxerat D. Collins.

Eruditi ex Academiâ Regiâ Parisiensi, auditâ D. Gregorii morte, cupide sciscitabantur ea, quæ moriens reliquerat ; simulque narrationem eorum, quæ attinent doctrinam serierum infinitarum, apud nos repertam petebant : sequentem ideo ad eos transmittendam curavi, ac deinde ad Davidem Gregorium, fratrem Jacobi superstitem.

+ Opusculum prædictum de Quadratura Arithmetica D. Leibnitius polire perrexit.

X x x 2

Quod

Quod attinet doctrinam serierum infinitarum, Mercator, in Logarithmo-techniâ suâ, primum specimen ejus orbi exhibuit; applicando eam ad hyperbolæ quadraturam tantum, & ad logarithmorum constructionem, abique radicum extractione. Hanc ipsam ejus doctrinam à D. Wallisio, in Trans. Philosoph. illustratam habemus; eamque postea adauxit & promovit D. Gregorius in Exercitationibus ejus Geometricis, eodem anno editis.

Paucos post menses quàm editi sunt hi libri, missi sunt ad D. Barrovium Cantabrigiæ. Ille autem responsum dedit, hanc infinitarum serierum doctrinam jam ante biennium à D. Isaaco Newtono inventam fuisse; & quibusvis figuris generaliter applicatam: simulque transmisit D. Newtoni opus manuscriptum, à D. Collins deinde cum D. Vicecomite Brounker, Regiæ Societatis tum Præside, communicatum. Barrovio autem cathedram mathematicam abdicante, Newtonus, ab eodem commendatus, in successorem ejus electus est; & de hac doctrinâ publicè prælegit: lectionesque ejus in bibliothecâ publicâ Cantabrigiensi asservantur.

Collins deinde, mediante D. Barrovio D. Newtono familiaris factus, literarum commercium cum eo habuit; & ab eo epistolam obtinuit 10 Decembris, anno 1672 datam, quâ docet modum ducendi tangentes ad Curvas geometricas, ope æquationis, quâ relatio inter ordinatim applicatas & abscissas exprimitur. Vide epistolam hanc, N° XXVI.

Collins etiam in diversis literis, anno 1669 ad D. Gregorium datis, eidem significavit Newtoni in hac materiâ successus. Gregorius autem contra, se quoque plures habere pro circulo series; simulque petiit nonnullas à Newtonianis, quas cum propriis conferre voluit, ad se mitti. Misit igitur aliquas D. Collins; quas Gregorius à suis prorsus diversas, & faciliores calculoque aptiores inveniens, haud levi studio in eandem ipsam Newtoni methodum incidit, circa annum exeuntem 1670: sicut ipse apertè in epistolâ 19 Decemb. testatur. N° XVIII.

Cùm D. Leibnitijs *methodum perveniendi ad series, anno superiori sibi missas desideraret, & ut Gregoriana omnia Lutetiam Parisiorum mitterentur*: Oldenburgus & Collins, Newtonum enixè rogârunt, ut ipse *methodum suam describeret cum D. Leibnitio communicandam.*

N° XLVIII. *Epistola prior D. Isaaci Newtoni, Matheseos Professoris in celeberrimâ Academiâ Cantabrigiensi, ad D. Henricum Oldenburg, Regalis Societatis Londini Secretarium, 13 Junii 1676; cum illustrissimo viro D. Godfredo Gulielmo Leibnitio (eo mediante) communicanda. Literis Oldenburgi (26 Junii) ad Leibnitium missa.*

NEWTONI
AD OLDEN-

QUAMQUAM D. Leibnitii modestia, in excerptis quæ ex epistolâ ejus ad me nuper misisti, nostratibus multum tribuat circa speculationem quandam Infinitarum Serierum, de quâ jam cœpit esse rumor: nullus dubito tamen quin ille, non tantum, quod asserit, methodum reducendi quantitates quæ-

cunque in ejusmodi series, sed & varia compendia, fortè nostris familia, si QUIBUS PR. OR. non & meliora, adinvenit.

Quoniam tamen ea scire pervelit, quæ ab Anglis hæc in re inventa sunt; & ipse ante annos aliquot in hanc speculationem inciderim: ut votis ejus aliquâ saltem ex parte satisfacerem, nonnulla eorum, quæ mihi occurrerunt, ad te transmissi.

Fractiones in infinitas series reducuntur per divisionem; & quantitates radicales, per extractionem radicum; perinde instituendo operationes istas in speciebus, ac institui solent in decimalibus numeris. Hæc sunt fundamenta harum reductionum.

Sed extractiones radicum multum abbreviantur per hoc * Theorema.

$$\sqrt[m]{P + PQ} = P^{\frac{1}{m}} + \frac{1}{m} A Q + \frac{m-1}{2m} B Q^2 + \frac{m-2}{3m} C Q^3 + \frac{m-3}{4m} D Q^4 + \&c.$$

Ubi $P + PQ$ significat quantitatem, cujus radix, vel etiam dimensio quævis, vel radix dimensionis, investiganda est; P , primum terminum quantitatis ejus; Q , reliquos terminos divisos per primum. Et $\frac{1}{m}$, numeralem indicem dimensionis ipsius $P + PQ$: sive dimensio illa integra sit; sive, ut ita loquar, fracta; sive affirmativa, sive negativa. Nam, sicut analysiæ, pro aa , aaa , &c. scribere solent a^2 , a^3 , &c. sic ego, pro \sqrt{a} , $\sqrt[3]{a}$, $\sqrt[4]{a}$, &c. scribo $a^{\frac{1}{2}}$, $a^{\frac{1}{3}}$, $a^{\frac{1}{4}}$, scribo a^{-1} , a^{-2} , a^{-3} . Et sic pro $\frac{aa}{\sqrt{c \cdot a^3 + bba}}$, scribo $aa \times a^{\frac{1}{2}} + bba)^{-\frac{1}{2}}$; & pro $\frac{aab}{\sqrt{c \cdot a^3 + bba \times a^3 + bba}}$: scribo $aab \times a^{\frac{1}{2}} + bba)^{-\frac{1}{2}}$. In quo ultimo casu, si $a^{\frac{1}{2}} + bba)^{-\frac{1}{2}}$ concipiatur esse $\sqrt[m]{P + PQ}$ in regulâ: erit $P = a^2$; $Q = \frac{bbx}{a^3}$; $m = -2$; $n = 3$. Denique, pro terminis inter operandum inventis in quoto, usurpo A , B , C , D , &c. Nempe A , pro primo termino $P^{\frac{1}{m}}$; B , pro secundo $\frac{1}{m} A Q$; & sic deinceps. Cæterum usus regulæ patebit exemplis.

Exemplum 1. Est $\sqrt{cc + xx}$ (seu $cc + xx)^{\frac{1}{2}}$ = $c + \frac{xx}{2c} - \frac{x^4}{8c^3} + \frac{x^6}{16c^5} - \frac{5x^8}{127c^7} + \&c.$ Nam, in hoc casu, est $P = cc$; $Q = \frac{xx}{c}$; $m = 1$; $n = 2$; $A (= P^{\frac{1}{m}} = cc^{\frac{1}{2}}) = c$. $B (= \frac{1}{m} A Q) = \frac{xx}{2c}$. $C (= \frac{m-1}{2m} B Q) = -\frac{x^4}{8c^3}$. Et sic deinceps.

Exemplum 2. Est $\sqrt{5 : c^3 + c^4 x - x^3}$ (id est, $c^3 + c^4 x - x^3)^{\frac{1}{2}}$ = $c + \frac{c^4 x - x^3}{2c^3} - \frac{c^8 x^2 - 2c^4 x^3 + 4c^4 x^5 - 2x^6}{5c^6} + \&c.$ Ut patebit substituendo in allatam regulam, 1 pro m , 5 pro n , c^3 pro P , & $\frac{c^4 x - x^3}{c^3}$ pro Q . Potest etiam $-x^3$ substitui pro P , & $\frac{c^4 x + c^3}{-x^3}$ pro Q . Et tunc evadet $\sqrt{5 : c + c^4 x - x^3} = -x +$

* Resolutionem binomii in hujusmodi seriem anno 1669 Newtono innotuisse patet, ex analysi suprà impressâ. Vid. suprà, N° XII.

NEWTONI
AD OLDEN- $\frac{c^2x + c^3}{5x^4} + \frac{2c^2xy + 4c^2x + 2c^{10}}{25x^9} + \&c.$ Prior modus eligendus est, si x valdè parvum sit; posterior, si valdè magnum.

Exem. 3. Est $\sqrt[n]{c^2y^2 - ay}$ (hoc est, $N \times y^2 - ay)^{-\frac{1}{n}}$ æqualis $N \times \frac{1}{y} + \frac{aa}{3y^3} + \frac{2a^2}{9y^5} + \frac{14a^3}{81y^7} + \&c.$ Nam $P = y^3$. $Q = -\frac{aa}{yy}$. $m = -1$, $n = 3$. $A (P^{\frac{m}{n}} = y^3 \times -\frac{1}{3}) = y^{-1}$, hoc est $\frac{1}{y}$. $B (= \frac{m}{n} A Q = -\frac{1}{3} \times \frac{1}{y} \times -\frac{aa}{yy}) = \frac{aa}{3y^3} + \&c.$

Exem. 4. Radix cubica ex quadrato-quadrato ipsius $d+e$, hoc est $\sqrt[3]{(d+e)^2}$, est $d^{\frac{2}{3}} + \frac{4ed^{\frac{1}{3}}}{3} + \frac{2ee}{9d^{\frac{2}{3}}} - \frac{4e^2}{81d^{\frac{1}{3}}} + \&c.$ Nam $P = d$. $Q = \frac{e}{d}$. $m = 4$. $n = 3$.

$A (P^{\frac{m}{n}} = d^{\frac{4}{3}})$, &c.

Exem. 5. Eodem modo simplices etiam potestates eliciuntur. Ut, si quadrato-cubus ipsius $d+e$, (hoc est, $\sqrt[3]{(d+e)^2}$, seu $\sqrt[3]{d+e}$) desideretur. Erit juxta regulam, $P = d$. $Q = \frac{e}{d}$. $m = 5$. & $n = 1$. Adeoque $A (P^{\frac{m}{n}} = d^5)$. $B (= \frac{m}{n} A Q) = 5d^4e$. & sic $C = 10d^3ee$. $D = 10dde^2$. $E = 5de^3$. $F = e^5$. & $G (= \frac{m-5n}{6n} PQ) = 0$. Hoc est, $\sqrt[3]{d+e} = d^{\frac{1}{3}} + 5d^{\frac{2}{3}}e + 10d^{\frac{1}{3}}ee + 10dde^2 + 5de^3 + e^5$.

Exem. 6. Quinetiam divisio, five simplex sit, five repetita, per eandem regulam perficitur. Ut si $\frac{1}{d+e}$, hoc est, $(d+e)^{-1}$, five $\sqrt[5]{d+e}^{-1}$, in seriem simplicium terminorum resolvendum sit: erit, juxta regulam, $P = d$. $Q = \frac{e}{d}$. $m = -1$. $n = 1$. & $A (P^{\frac{m}{n}} = d^{-1}) = d^{-1}$, seu $\frac{1}{d}$. $B (= \frac{m}{n} A Q = -1 \times \frac{1}{d} \times \frac{e}{d}) = -\frac{e}{dd}$. Et sic $C = \frac{ee}{d^3}$. $D = -\frac{e^2}{d^4}$, &c. Hoc est, $\frac{1}{d+e} = \frac{1}{d} - \frac{e}{dd} + \frac{ee}{d^3} - \frac{e^2}{d^4} + \&c.$

Exem. 7. Sic & $\sqrt[3]{d+e}^{-3}$, hoc est unitas ter divisa per $d+e$, vel semel per cubum ejus, evadit $\frac{1}{d^3} - \frac{3e}{d^4} + \frac{6ee}{d^5} - \frac{10e^2}{d^6} + \&c.$

Exem. 8. Et $N \times \sqrt[3]{d+e}^{-\frac{1}{3}}$, hoc est N divisum per radicem cubicam ipsius $d+e$, evadit $N \times \frac{1}{d^{\frac{1}{3}}} - \frac{e}{3d^{\frac{4}{3}}} + \frac{2ee}{9d^{\frac{5}{3}}} - \frac{14e^2}{81d^{\frac{7}{3}}} + \&c.$

Exem. 9. Et $N \times \sqrt[3]{d+e}^{-\frac{1}{3}}$, hoc est N divisum per radicem quadrato-cubicam ex cubo ipsius $d+e$, five $\sqrt[3]{5: d^3 + 3dde + 3dee + e^3}$, evadit $N \times \frac{1}{d^{\frac{1}{3}}} - \frac{3e}{5d^{\frac{4}{3}}} + \frac{12ee}{25d^{\frac{5}{3}}} - \frac{52e^2}{125d^{\frac{7}{3}}} + \&c.$

Per eandem regulam, geneses potestatum, divisiones per potestates aut per quantitates radicales, & extractiones radicum altiorum in numeris, etiam commodè instituuntur.

Extractiones

Extractiones radicum æquationum affectarum in speciebus imitantur earum extractionem in numeris. Sed methodus Vietæ & Oughtredi nostri huic negotio minùs idonea est. Quapropter aliam excogitare adactus sum, cujus specimina, ne repetantur, vide in Tractatu de Analyt. pag. &c. &c.

Dicam tantum in genere, quòd radix cujusvis æquationis semel extracta, pro regulâ resolvendi consimiles æquationes afferrari possit; quòdque ex pluribus ejusmodi regulis, regulam generaliore[m] plerumque efformare liceat; & quòd radices omnes, five simplices sint, five affectæ, modis infinitis extrahi possint; de quorum simplicioribus itaque semper consulendum est.

Quomodo ex æquationibus, sic ad infinitas series reductis, areæ & longi-^{Nº XLIX.} tudines Curvarum, contenta & Superficies solidorum, vel quorumque segmentorum figurarum quarumvis, eorumlibet centra gravitatis determinentur; & quomodo etiam Curvæ omnes Mechanicæ ad ejusmodi æquationes infinitarum serierum reduci possint; indeque problemata circa illas solvi, perinde ac si Geometricæ essent; nimis longum fore describere. Sufficiat specimina quædam talium problematum recensuisse: inque iis, brevitatis gratiâ, literas A, B, C, D, &c. pro terminis seriei, sicut sub initio, nonnunquam usurpabo.

1. Si ex dato sinu recto, vel sinu verso, arcus desideretur: sit radius r , & sinus rectus x . Eritque arcus $= x + \frac{x^3}{6rr} + \frac{3x^5}{40r^3} + \frac{5x^7}{112r^5} + \&c.$ Hoc est, $= x + \frac{1 \times x \times x}{2 \times 3 \times rr} A + \frac{3 \times 3 \times x}{4 \times 5 \times rr} B + \frac{5 \times 5 \times x}{6 \times 7 \times rr} C + \frac{7 \times 7 \times x}{8 \times 9 \times rr} D + \&c.$ Vel sit d diameter, ac x sinus versus; & erit arcus $= d^{\frac{1}{2}} \lambda^{\frac{1}{2}} + \frac{x^3}{6d^{\frac{3}{2}}} + \frac{3x^5}{40d^{\frac{5}{2}}} + \frac{5x^7}{112d^{\frac{7}{2}}} + \&c.$ Hoc est, $= \sqrt{dx}$ in $1 + \frac{x}{6d} + \frac{3x^2}{40dd} + \frac{5x^3}{112dd^2} + \&c.$

2. Si vicissim ex dato arcu desideretur sinus: sit radius r , & arcus z . Eritque sinus rectus $= z - \frac{z^3}{6rr} + \frac{z^5}{120r^3} - \frac{z^7}{5040r^5} + \frac{z^9}{362880r^7} - \&c.$ Hoc est, $= z - \frac{zz}{2 \times 3 \times rr} A - \frac{zz}{4 \times 5 \times rr} B - \frac{zz}{6 \times 7 \times rr} C - \&c.$ Et sinus versus $= \frac{zz}{2r} - \frac{z^3}{24r^3} + \frac{z^5}{720r^5} - \frac{z^7}{40320r^7} + \&c.$ Hoc est, $\frac{zz}{1 \times 2 \times r} A - \frac{zz}{3 \times 4 \times rr} B - \frac{zz}{5 \times 6 \times rr} C - \frac{zz}{7 \times 8 \times rr} D - \&c.$

3. Si arcus capiendus sit in ratione datâ ad alium arcum: esto diameter $= d$; chorda arcus dati $= x$; & arcus quæsitus ad arcum illum datum ut n ad 1. Eritque arcus quæsitus chorda $= nx + \frac{1-nn}{2 \times 3dd} xxA + \frac{9-nn}{4 \times 5dd} xxB + \frac{25-nn}{6 \times 7dd} xxC + \frac{49-nn}{8 \times 9dd} xxD + \frac{81-nn}{10 \times 11dd} xxE + \&c.$

Ubi nota, quòd cum n est numerus impar, series desinet esse infinita; & evadet eadem, quæ prodit per vulgarem Algebram, ad multiplicandum datum angulum per istum numerum n .

$\frac{1^3}{1 \times 2} + \frac{1^3}{1 \times 2 \times 3} + \frac{1^3}{1 \times 2 \times 3 \times 4} \&c.$ Prior tamen celerius appropinquat. Ideoque efficio, ut eâ possim uti, etiam cum major est unitate numerus $1+n$. Nam idem est logarithmus pro $1+n$ & pro $\frac{1}{1+n}$. Unde, si $1+n$ sit major unitate, erit $\frac{1}{1+n}$ minor unitate. Fiat ergo $1+m = \frac{1}{1+n}$, ac inventa m , habebitur & $1+n$ numerus quaesitus.

Quod regressum ex arcubus attinet *, incideram ego directe in regulam, quæ ex dato arcu sinum complementi exhibet. Nempe, sinus complementi $= 1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4} \&c.$ Sed postea quoque deprehendi, ex eâ illam nobis communicatam, pro inveniendâ sinu recto, qui est $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} \&c.$ posse demonstrari.

Quod tribus verbis sic fit. Summa sinuum complementi ad arcum, seu omnium $1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4} \&c.$ est $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} \&c.$ Porro, summa sinuum complementi ad arcum, seu arcui in locis debitis insistentium, æquatur sinui recto ducto in radium, ut notum est geometricis; id est, æquatur ipsi sinui recto; quia radius hic est unitas. Ergo sinus rectus $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} \&c.$ Hinc etiam, ex dato arcu & radio, sine ullâ prorsus aliorum notitiâ, haberi potest area segmenti circularis duplicati: quæ est $\frac{a^2}{1 \times 2 \times 3} - \frac{a^4}{1 \times 2 \times 3 \times 4 \times 5} + \frac{a^6}{1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7} \&c.$ Unde optimè segmentorum tabula ad gradus & minuta, &c. calculabitur.

Pro trigonometricis autem operationibus, percommoda mihi videtur hæc expressio: ut sinus complementi, c , ponatur $= 1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4}$; quoniam sola, memoriâ retenta, omnibus casibus & operationibus, directis scilicet simul & reciprocis, sufficit; quod ideo fit, quoniam æquatio $c = 1 - \frac{a^2}{2} + \frac{a^4}{24}$ est plana. Unde si vicissim quæras arcum ex sinu complementi, radix extrahi potest; adeoque fiet arcus $a = \sqrt{6 - \sqrt{24c + 12}}$ exactè satis ad usum eorum, qui in itineribus tabularum commoditate carent; quia error æquationis non est $\frac{a^6}{720}$.

Innumera alia possent dici, quæ his fortasse elegantia & exactitudine non cederent. Sed ego ita sum comparatus, ut plerumque, methodis generalibus detectis, rem in potestate habere contentus, reliqua libenter aliis relinquam. Neque enim ista omnia magnopere æstimanda sunt, nisi quod artem inveniendi perficiunt, mentemque excolunt. Si quæ obscuriora videntur, ea libenter elucidabo: et illud quoque explicabo, quomodo hæc methodo

* N. B. Methodum perveniendi ad has series Leibnitius à Newtono jam modo acceperat, idque ex ipsius rogatu. Imo series ipsæ à Newtono, unâ cum methodo perveniendi ad easdem modo

methodo æquationum quoque utcunque affectarum radices per infinitam seriem dari possint, sine ullâ extractione; quod mirum fortasse videbitur.

Sed desideraverim, ut Clarissimus Newtonus nonnulla quoque amplius explicet; ut originem theorematis, quod initio ponit: item, modum quo quantitates p, q, r , in suis operationibus invenit: ac denique, quomodo in methodo regressuum se gerat; ut cum ex logarithmo quærit numerum. Neque enim explicat, quomodo id ex methodo suâ derivetur.

Nondum mihi licuit, ejus literas, quâ merentur diligentia, legere: quoniam tibi è vestigio respondere volui. Unde non satis nunc quidem affirmare ausim, an nonnulla eorum quæ suppressit, ex solâ earum lectione consequi possum. Sed optandum tamen foret, ipsum ea potius supplere Newtonum: quia credibile est, non posse eum scribere, quin aliquid semper præclari nos doceat, ut apparet, egregiarum meditationum plenus.

Ad alia tuarum literarum venio; quæ doctissimus Collinius communi-^{Nº LIII.} care gravatus non est. Vellem adjecisset appropinquationis Gregorianæ linearis demonstrationem. Credo tamen aliam haberi simpliciorē, etiam in infinitum euntē; quæ fiat sine ullâ bisectione anguli; imo sine suppositâ circuli constructione; solo rectarum ductu.

Vellem Gregoriana omnia conservari. Fuit enim his certè studiis promovendis aptissimus: ceterum ejus demonstrationi editæ, de impossibilitate quadraturæ absolutæ circuli & hyperbolæ, multa haud dubiè defunt.

De æquationum radicibus surdis generalibus inveniendis; five, quod idem est, tollendis æquationum potestatibus intermediis, multa & ego meditatus sum; & jam vere anni superioris, specimina Hugenio communicaveram regularum Cardanicis similibus. Seriem enim habebam ejusmodi regularum in infinitum euntē; in quibus & Cardanica continebatur. Sed ultra gradum cubicum non erant generales. Perspexi tamen inde veram methodum progrediendi longius. Quamquam multis adhuc opus sit artibus, quas excutiendas libenter ingeniosissimo Tschurnhausio relinquo; qui hic ad eadem quæ ego habebam specimina, imo & alia præterea, etiam de suo pervenit.

Ex iis quæ Collinius ait de Gregorianâ methodo, difficile non fuit nobis certo divinare, in quo consistat ejus substantia.

Imaginariorum quantitatum, in realium radicum expressiones ingredientium, sublationem frustra putem sperari, imo quæri. Neque enim illæ ullo modo vel calculis vel constructionibus obsunt: et veræ realesque sunt quantitates, si inter se conjunguntur, ob destructiones virtuales. Quod multis elegantibus exemplis, & argumentis, deprehendi.

Exempli gratiâ, $\sqrt{1+\sqrt{-3}} + \sqrt{1-\sqrt{-3}} = \sqrt{6}$. Tametsi enim neque ex binomio $\sqrt{1+\sqrt{-3}}$, neque ex binomio $\sqrt{1-\sqrt{-3}}$ radix extrahatur; nec proinde sic destruetur imaginaria $\sqrt{-3}$: supponenda tamen est destructa esse virtualiter; quod actu appareret, si fieri posset extractio. Aliâ tamen viâ hæc summa reperitur esse $\sqrt{6}$. Unde in cubicis binomiis, ubi realitas ejusmodi formularum, tunc cum extractio ex singulis binomiis fieri

modo acceperat, & pro hyperbolâ signum tantum mutavit; pro circulo, sinum versum à Newtono acceptum subduxit à radio, ut haberet sinum complementi.

nequit, ad oculum ostendi non potest, mente tamen intelligitur. Quare frustra Cartesius alique expressiones Cardanicas pro particularibus habuere. Siquis posset invenire quadraturam circuli & ejus partium, ex datâ hyperbolæ & ejus partium quadraturâ, is posset eas tollere; modò in ipsam quadraturam imaginariâ illæ rursus ingrediantur.

Cæterum ex illis, quas habeo, meditationibus circa radices æquationum irrationales, necessario sequitur res satis paradoxa: scilicet omnes æquationes gradûs octavi, noni, decimi, posset ad gradum septimum reduci. Itaque & omnia problemata, ad decimum gradum usque occurrentia, possunt ad septimum deprimi.

Horribiles calculi fubeundi erunt illi, qui in hoc argumentum velut per vim irrumpet; sed facilîmi, ipsi qui autè meditabitur: cùm, ut prævideo, ipsa natura rei ducat ad compendia quædam, per quæ spes est calculi magnam partem abscindi; remque elegantibus artificiis, ingenii potius vi quàm calculi labore, transigi posse.

Sed si quis laborem non subterfugeret, eum docere possum methodum analyticam generalem infallibilem, per quam omnium æquationum radices generales invenire liceret.

Verùm meliora illis proponerem agenda, qui calculo delectarentur. Consilium enim habeo tabularum analyticarum, quæ non minoris futuræ essent usûs in Analyti, quàm tabulæ sinuum in Geometriâ Practicâ; imo, arbitror, qui paulum in iis calculandis versatus sit, eum progressionem reperiaturum in infinitum, quarum ope magna tabulæ pars sine labore continuari possit. Nihil est, quod nôrim, in totâ analysi momenti majoris. Nam in his tabulis pleraque problemata statim soluta haberentur, aut levi operâ possint inde deduci.

Pendet negotium ex re longè majore; arte scilicet combinatoriâ generali ac verâ. Cujus vim ac potestatem nescio, an quisquam hætenus sit contempnitus. Ea verò nihil differt ab analysi illâ supremâ, ad cujus intima, quantum judicare possum, Cartesius non pervenit. Est enim, ad eam constituendam, opus alphabeto cogitationum humanarum. Et ad inventionem ejus alphabeti, opus est analysi axiomatum. Sed non miror ista nemini satis considerata: quia plerumque facilia negligimus; & multa, quæ clara videntur, assumimus. Quam quamdiu faciemus, nunquam ad illud pervenimus, quod mihi videtur in rebus intellectualibus summum; nec genus calculi, etiam non-mathematicis accommodati, obtinebimus.

Optârîm Cl. Pellium generalia sua meditata, & illud speciatim, quod memoras, Cribrum Eratosthenis non suppressere. Nam etsi omnia fortè, quæ destinârat, non absolverit; meditata tamen ipsa, & consilia, egregiorum virorum non perire, publici interest. Utilia quoque futura sunt, quæ de sinuum tabulâ ad æquationes accommodandâ habet. Item de limitibus, & radicibus.

Quod dicere videmini, plerasque difficultates, exceptis problematibus Diophantæis, ad series infinitas reduci; id mihi non videtur. Sunt enim multa usque adeo mira & implexa, ut neque ab æquationibus pendeant, neque

neque ex quadraturis. Qualia sunt, ex multis aliis, problemata* methodi tangentium inversæ; quæ etiam Cartesius in potestate non esse fatetur.

In Tomo III Epistolarum, una habetur ad Beaunium; in quâ, ad propositas à Beaunio, Curvas quasdam invenire conatur; quarum una est ludus naturæ: ut intervallum inter tangentem, ad [axem] directricem usque productam, & ordinatim-applicatam ex Curvâ ad directricem, sit semper idem; recta scilicet constans. Hanc Curvam nec Cartesius, nec Beaunius, nec quisquam alius, quod sciam, invenit. Ego verò quâ primum die, imo horâ, cæpi quærere, statim certâ analysi solvi. Fateor tamen nondum me, quicquid in hoc genere desiderari potest consecutum: quamquam maximi momenti esse facias. Ac de his quidem nunc satis.

Ego id agere constitui, ubi primum otium nactus ero, ut rem omnem mechanicam reducam ad puram geometriam; problemataque circa elateria, & aquas, & pendula, & projecta, & Solidorum resistantiam, & fractiones, &c. definiam. Quæ hætenus attingit nemo. Credo autem rem omnem nunc esse in potestate; ex quo circa regulas motuum mihi penitus perfectis demonstrationibus satisfeci; neque quicquam amplius in eo genere desidero. Tota autem res, quod mireris, pendet ex axioma metaphysico pulcherrimo; quod non minoris est momenti circa motum, quam hoc, *totum esse majus parte*, circa magnitudinem.

De centro-baricis quoque, singularem quendam aditum reperi ad novas ac plane à prioribus diversas contemplationes, in Geometriâ pariter ac Mechanicâ magno usui futuras. Hæc ubi (Deo volente) absolvero, reliquum temporis, quod scilicet philosophicis meditationibus destinare fas erit, naturæ indagationi debeo.

Tschurnhausius proximo tabellione scribet.

Excerpta ex epistola D. Ehrenfried de Tschurnhaufe ad D. Oldenburgum, N° LIV.

Parisiis 1° Septemb. 1676 datâ; cujus extat exemplar manu D. Collinis descriptum.

Expectabam cum desiderio responsum, cùm aliquot abhinc mensibus ad te literas meas transmisseram; sed nec ex modò datis colligere licet, has receptas fuisse. Interim admodum oblectatus fui, hisce conspectis quæ ad D. Leibnitium exarâsti; maximeque me tibi devinxisti, quòd me participem volueris facere tam ingeniosarum inventionum, & promotionis geometriæ tam pulchræ quàm utilis. Statim cursim eas pervolvi, ut viderem, num forte inter hæc series infinitas existeret + ea, quâ ingeniosissimus D. Leibnitius circulum, imo quamvis sectionem conicam (centro in finitâ distantia gaudentem) quadravit; tali ratione, ut mihi persuadeam simpliciorrem viam, nec quoad linearum constructionem, nec numeralem expressio-

* Si æquationes differentiales D. Leibnitio jam innotuissent, haud dixisset, problemata methodi tangentium inversæ ab æquationibus non pendere.

+ Annon D. Tschurnhaufe viderat excerpta ex Gregorii epistolis, cum D. Leibnitio communicata; ubi habetur series Gregorii, quam Leibnitio hic tribuit? Vide pag 221.

nem, nunquam visum iri; quique hisce porro insistent, generalem advenit methodum figuram quamvis datam in talem rationalem transmutandi, quæ per solum inventum (admodum præstans meo iudicio) D. Mercatoris, ad seriem infinitam posset reduci. Sed hæc de materiâ, cum ipse non ita pridem mentem suam declaravit, non opus est ut prolixior sim. Verum ut ad specimina perquam ingeniosa D. Newtoni revertar; hæc non potuere non mihi placere; tam ob utilitatem, quâ se tam latè ad quarumvis quantitatum dimensiones, ac alia difficilia enodanda in mathematicis extendunt, quàm ob deductionem harum, à fundamentis non minus generalibus, quàm ingeniosis, derivatam: non obstante quod existimem, ad quantitatem quamvis ad infinitam seriem æquipollentem reducendam, fundamenta adhuc dari & simpliciora & universaliora, quàm sunt fractionum & irrationalium reductio ad tales series, ope divisionis aut extractionis; quæ mihi tale quid non nisi per accidens præstare videntur: cum hæc successum quoque habeant, licet non adsint fractiones aut irrationales quantitates. Similia porro, quæ in hac re præstitit eximius ille geometra Gregorius, memoranda certè sunt; & quidem optimè famæ ipsius consulturi, qui ipsius relicta manuscripta luci publicæ ut exponantur operam navabunt.

Nº LV. *Epistola D. Newtoni posterior, ad D. Oldenburgum, Octob. 24, 1676 data, cum D. Leibnitio communicanda.*

Vir Dignissime,

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QUANTA cum voluptate legi epistolas clarissimorum virorum D. Leibnitii & D. Tschurnhausii, vix dixerim.

Perelegans sane est Leibnitii methodus perveniendi ad series convergentes: & satis ostendisset ingenium authoris, etli nihil aliud scripsisset. Sed quæ alibi per epistolam sparsit, suo nomine dignissima, efficiunt etiam, ut ab eo speremus maxima Diversitas modorum, quibus eodem tenditur, eo magis placuit, quod mihi tres methodi perveniendi ad ejusmodi series innotuerant; adeo ut novam nobiscum communicandam vix expectarem.

Unam è meis prius descripsi: jam addo aliam; illam scilicet quâ primum incidi in has series. Nam incidi in eas, antequam scirem divisiones & extractiones radicum, quibus jam utor. Et hujus explicatione pandendum est fundamentum theorematum, sub initio epistolæ prioris positi, quod D. Leibnitius à me desiderat.

Sub initio studiorum meorum mathematicorum, ubi incideram in Opera celeberrimi Wallisii nostri: considerando series, quarum intercalatione ipse exhibet aream circuli & hyperbolæ; utpote quod in serie Curvarum, quarum basis seu axis communis sit x , & ordinatim applicatæ $1 - xx^{\frac{1}{2}}$, $1 - xx^{\frac{1}{3}}$, $1 - xx^{\frac{1}{4}}$, $1 - xx^{\frac{1}{5}}$, $1 - xx^{\frac{1}{6}}$, $1 - xx^{\frac{1}{7}}$, &c. si areæ alternarum, quæ sunt x , $x - \frac{1}{3}x^3$, $x - \frac{2}{3}x^3 + \frac{1}{5}x^5$, $x - \frac{3}{5}x^3 + \frac{2}{7}x^5 - \frac{1}{7}x^7$ &c. interpolari possent; ha-

* Vide D. Wallisii arithmetica infinitorum, Prop. cxviii, cxxi, ejusque algebram, Cap. lxxxi.

beremus

beremus areas intermediarum, quarum prima, $1 - xx^{\frac{1}{2}}$, est circulus: ad has ^{PRIMUM} interpolandas notabam, quod in omnibus, primus terminus esset x ; quod- ^{POSTERIOR.} secundi termini, $\frac{0}{3}x^{\frac{1}{2}}$, $\frac{1}{3}x^{\frac{1}{2}}$, $\frac{2}{3}x^{\frac{1}{2}}$, $\frac{3}{3}x^{\frac{1}{2}}$, &c. essent in arithmetica progressionem; & proinde quod duo primi termini serierum intercalandarum deberent esse $x - \frac{1}{3}x^3$, $x - \frac{2}{3}x^3$, $x - \frac{3}{3}x^3$, &c.

Ad reliquas intercalandas considerabam, quod denominatores, 1, 3, 5, 7, &c. erant in arithmetica progressionem; adeoque solæ numeratorum coefficientes numerales essent investigandæ. Hæ autem in alternis datis areis erant figuræ potestatum numeri undenarii; nempe 11^0 , 11^1 , 11^2 , 11^3 , 11^4 . Hoc est, primo 1; deinde 1, 1; tertio 1, 2, 1; quarto 1, 3, 3, 1; quinto 1, 4, 6, 4, 1, &c.

Quærebam itaque, quomodo in his seriebus, ex datis duabus primis figuris, reliquæ derivari possent. Et inveni, quod posita secundâ figurâ m , reliquæ producerentur per continuam multiplicationem terminorum hujus seriei, $\frac{m-0}{1} \times \frac{m-1}{2} \times \frac{m-2}{3} \times \frac{m-3}{4} \times \frac{m-4}{5}$ &c.

Exempli gratiâ. Sit terminus secundus $m = 4$; & erit $4 \times \frac{m-1}{2}$, hoc est 6, tertius terminus; & $6 \times \frac{m-2}{3}$, hoc est 4, quartus; & $4 \times \frac{m-3}{4}$, hoc est 1, quintus; & $1 \times \frac{m-4}{5}$, hoc est 0, sextus; quo series in hoc casu terminatur.

Hanc regulam itaque applicui ad series interferendas. Et cum pro circulo secundus terminus esset $\frac{1}{3}x^{\frac{1}{2}}$, posui $m = \frac{1}{2}$: & prodierunt termini $\frac{1}{3} \times \frac{\frac{1}{2}-1}{2}$, five $-\frac{1}{8}$; $-\frac{1}{8} \times \frac{\frac{1}{2}-2}{3}$, five $+\frac{1}{16}$; $+\frac{1}{16} \times \frac{\frac{1}{2}-3}{4}$, five $-\frac{1}{128}$; & sic in infinitum. Unde cognovi, desideratam aream segmenti circularis esse $x - \frac{1}{3}x^{\frac{1}{2}}$, $\frac{1}{5}x^{\frac{1}{2}} - \frac{1}{75}x^{\frac{3}{2}} - \frac{1}{125}x^{\frac{5}{2}}$ &c.

Et eadem ratione prodierunt etiam interferendæ areæ reliquarum Curvarum: ut & area hyperbolæ, & cæterarum alternarum in hac serie; $1 + xx^{\frac{1}{2}}$, $1 + xx^{\frac{1}{3}}$, $1 + xx^{\frac{1}{4}}$, $1 + xx^{\frac{1}{5}}$, &c.

Et eadem est ratio intercalandi alias series; idque per intervalva duorum pluriumve terminorum simul deficientium.

Hic fuit primus meus ingressus in has meditationes: qui è memoriâ sane exciderat, nisi oculis in adversaria quædam, ante paucas septimanas, retulissim.

Ubi verò hæc didiceram; mox considerabam terminos $1 - xx^{\frac{1}{2}}$, $1 - xx^{\frac{1}{3}}$, $1 - xx^{\frac{1}{4}}$, $1 - xx^{\frac{1}{5}}$, &c. hoc est, 1, $1 - xx$, $1 - 2xx + x^2$, $1 - 3xx + 3x^2 - x^3$, &c. eodem modo interpolari posse, ac areas ab ipsis generatas: & ad hoc nihil aliud requiri, quàm omissionem denominatorum 1, 3, 5, 7, &c. in terminis experimentibus areas; hoc est, coefficientes terminorum quantitatis intercalandæ $1 - xx^{\frac{1}{2}}$, vel $1 - xx^{\frac{1}{3}}$, vel generaliter $1 - xx^{\frac{1}{n}}$, prodire per continuam

Eo ipso tamen tempore quo liber iste prodit, communicatum est per amicū D. Barrow, tunc Matheseos professorem Cantabrigiæ, cum D. Collinio *, compendium quoddam methodi harum serierum; in quo significaveram areas & longitudines Curvarum omnium, & Solidorum superficies & contenta, ex datis rectis, & vice versa, ex his datis rectas determinari posse: & methodum ibi indicatam illustraveram diversis seriebus.

Subortâ deinde inter nos epistolari consuetudine; D. Collinius, vir in rem mathematicam promovendam natus, non destitit suggerere, ut hæc publici juris facerem. Et ante annos quinque [1671], cum suadentibus amicis consilium ceperam edendi tractatum de refractione lucis, & coloribus, quem tunc in promptu habebam; cœpi de his seriebus iterum cogitare: & tractatum † de iis etiam conscripsi, ut utrumque simul ederem.

Sed, ex occasione telescopii catadioptrici, epistolâ ad te missâ, quâ breviter explicui conceptus meos de naturâ lucis; inopinatum quiddam effecit, ut mei interesse sentirem, ad te festinanter scribere de impressione istius epistolæ. Et subortæ statim, per diversorum epistolâ objectionibus aliisque refertis, crebræ interpellationes me prorsus à consilio deterruerunt; & effecerunt, ut me arguerem imprudentiæ, quod umbram captando, eatenus perdidieram quietem meam, rem prorsus substantialem.

Sub eo tempore Jacobus Gregorius, ex unicâ quâdam serie è meis, quam D. Collinius ad eum transmisserat, post multam considerationem, ut ad Collinium rescripsit, pervenit ad eandem methodum; & tractatum de eâ reliquit, quem speramus ab amicis ejus editum iri. Siquidem, pro ingenio quo pollebat, non potuit non adjicere de suo nova multa; quæ rei mathematicæ interest, ut non pereant.

Ipse autem tractatum meum non penitus absolveram, ubi destiti à proposito; neque in hunc diem mens rediit ad reliqua adjicienda. Deerat quippe pars ea, quâ decreveram explicare modum solvendi problemata, quæ ad quadraturas reduci nequeunt; licet aliquid de fundamentis ejus posuissim. Cæterum in tractatu isto, series infinitæ non magnam partem obtinebant.

Alia haud pauca congesti; inter quæ erat methodus ducendi tangentes, quam solertissimus Slusius ante annos duos tresve tecum communicavit; de quâ tu, suggerente Collinio, rescripsisti, eandem ‡ mihi etiam innotuisse. Diversâ ratione in eam incidimus. Nam res non eget demonstratione, prout ego operor. Habito meo fundamento, nemo potuit tangentes aliter ducere, nisi volens de rectâ viâ deviare.

Quinetiam non hîc hæretur ad æquationes radicalibus, unam vel utramque indefinitam quantitatem involventibus, utcunque affectas; sed absque aliquâ

* Analysin intelligit per æquationes infinitas suprâ impressam, de quâ vid. pag. 502, 503.

† Hujus tractatûs meminit D. Collins in epistolis duabus suprâ impressis, pag. 508. Et Newtonus in epist. supra impressâ, pag. 510.

‡ Vide epistolam Newtoni suprâ impressam, pag. 510.

§ Hoc est, datâ æquatione quotcunque fluentes quantitates involvente, fluxiones invenire; & vice versa. Prior pars problematis solvitur per regulam binomii, initio epistolæ superioris Newtonianæ traditam, & initio hujus demonstratam. Nam si terminus secundus binomii sit momentum ter-

mini

aliquâ talium æquationum reductione, quæ opus plerumque redderet immensum, tangens confestim ducitur. Et eodem modo se res habet in quæstionibus de Maximis & Minimis; aliisque quibusdam, de quibus jam non loquor.

Fundamentum harum operationum, satis obvium quidem, quoniam jam non possum explicationem ejus prosequi, sic potius celavi § *bacedæ13-eff713/9n404qrr4/9t12vz.*

Hoc fundamento conatus sum etiam reddere || speculationes de quadraturâ Curvarum simpliciores; pervenique ad theoremata quædam generaliora. Et, ut candidè agam, ecce primum theorema.

Ad Curvam aliquam sit $ds^2 = e + f x^2$ ordinatim applicata, termino ab-^{Nº LVIII.} scissâ, seu tasis, a normaliter insistenti: ubi literæ d, e, f denotant quilibet quantitates datas; & θ, η, λ indices potestatum, sive dignitatum, quantita-

tum quibus affixæ sunt. Fac $\frac{e+1}{\eta} = r$; $\lambda + r = s$; $\frac{d}{\sqrt{e+f x^2}} + 1 = Q$; & $r\eta - \eta = \pi$. Et area Curvæ erit Q in $\frac{x^2}{1} - \frac{r-1}{1-1} \times \frac{A}{f x^2} + \frac{r-1}{1-1} \times \frac{B}{f x^2} - \frac{r-1}{1-1} \times \frac{C}{f x^2} + \frac{r-1}{1-1} \times \frac{D}{f x^2}$ &c. literis A, B, C, D, &c. denotantibus terminos proximè antecede-

ntes; nempe A terminum $\frac{x^2}{1}$, B terminum $-\frac{r-1}{1-1} \times \frac{A}{f x^2}$ &c. Hæc series,

ubi r fractio est vel numerus negativus, continuatur in infinitum; ubi verò r integer est & affirmativus, continuatur ad tot terminos tantum, quot sunt unitates in eodem r ; & sic exhibet geometricam quadraturam Curvæ. Rem exemplis illustro.

Exemplum 1. Proponatur Parabola, ejus ordinatim applicata sit \sqrt{ax} . Hæc, in formam regulæ reducta, sit $x^2 \times 0 + ax^{\frac{1}{2}}$. Quare $d=1$; $\theta=0$; $e=0$; $f=a$; $\eta=1$; $\lambda=\frac{1}{2}$. Adeoque $r=1$; $s=1\frac{1}{2}$; $Q=\frac{1}{a} \times ax^{\frac{1}{2}}$ & $\pi=0$. Et erit area quæ sita $\frac{1}{a} \times ax^{\frac{1}{2}}$ in $\frac{1}{1\frac{1}{2}}$; hoc est, $\frac{2}{3} \sqrt{ax}$. Et sic in genere si cx^r ponatur ordinatim applicata, prodibit area $\frac{c}{r+1} x^{r+1}$.

Exem. 2. Sit ordinatim applicata $\frac{ax^2}{c^2 - 2ccx + x^2}$. Hæc per reductionem sit $a^2 x \times \frac{cc - xz}{-1 + ccx - x^2}$; vel etiam $a^2 x^{-1} \times \frac{-1 + ccx - x^2}{-1 + ccx - x^2}$. In priori casu est $d=a^2$; $\theta=1$; $e=cc$; $f=-1$; $\eta=2$; $\lambda=-2$. Adeoque $r=1$; $s=-1$; $Q=-\frac{a^2}{-1} \times \frac{cc - xz}{-1 + ccx - x^2}$, hoc est $-\frac{a^2}{2cc - 2xz}$; $\pi=0$. Et area curvæ Q in

mini primi; terminus secundus seriei, in quam dignitas Binomii per regulam illam resolvitur, erit momentum dignitatis Binomii. Posterior pars problematis solvitur regrediendo à momentis ad fluentes: quod ubi hæretur, fieri solet quadrando figuras; & ubi ad quadraturas hæretur, extrahendo fluentes per regulas quatuor; quarum duas Newtonus in epistolâ priore explicuit; duas illas, sub finem hujus epistolæ, literis transpositis occultavit; ut mox dicetur.

¶ Hujusmodi theoremata Newtono ante annum 1669 innotuisse patet, per analysin suprâ impressam pag. 503, lin. 2, & per epistolam Collinii ad Thomam Storde 26 Julii 1671 datâ, pag. 509, lin. 24, 25, 26, ut & per hanc epistolam.

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$-\frac{x^2}{1}$, id est $= \frac{a^4}{2cx - 2xz}$. In secundo autem casu, est $d = a^4$; $\theta = -3$;
 $e = -1$; $f = cc$; $\eta = -2$; $\lambda = -2$; $r = 1$; $s = -1$; $Q = -\frac{a^4}{2cx}$.
 $-\frac{1}{1 + ccx^{-2}}$, id est $-\frac{a^4xz}{2c^2 - 2ccxz}$; $w = 0$. Et area $= Q$ in $-\frac{x^2}{1}$, hoc est

$\frac{a^4xz}{2c^2 - 2ccxz}$ Area his casibus diversimodè exhibetur, quatenus computatur à
 diversis finibus; quorum assignatio, per hos inventos valores arearum, fa-
 cilis est.

Exem. 3. Sit ordinata applicata $\frac{a^4}{x}\sqrt{bx + xz}$: hoc est, per reductio-
 nem ad debitam formam; vel $a^4x^{-1} \times \sqrt{b + z}$; vel $a^4x^{-1} \times \sqrt{1 + bz^{-1}}$. Et
 erit, in priori casu, $d = a^4$; $\theta = -\frac{1}{2}$; $e = b$; $f = 1$; $\eta = 1$; $\lambda = \frac{1}{2}$. Adeo-
 que $r = -\frac{1}{2}$, &c. Quare, cum r non sit numerus affirmativus, procedo
 ad alterum casum. Hic est $d = a^4$; $\theta = -4$; $e = 1$; $f = b$; $\eta = -1$;
 $\lambda = \frac{1}{2}$. Adeoque $r = 3$; $s = 3\frac{1}{2}$; $Q = -\frac{a^4}{b} \times \sqrt{1 + bz^{-1}}$, seu

$-\frac{a^4x + a^4b}{bzx} \sqrt{zx + bz}$; $\pi = -2$. Et area, Q in $\frac{x^{-1}}{3\frac{1}{2}} - \frac{2}{2\frac{1}{2}} \times \frac{x^{-1}}{3\frac{1}{2}b} + \frac{1}{1\frac{1}{2}} \times \frac{2}{2\frac{1}{2}} \times$
 $\frac{x^2}{3\frac{1}{2}bb}$; hoc est $\frac{-30bb^2 + 24bz - 16xz}{105bbzx}$ in $\frac{a^4x + a^4b}{bzx} \sqrt{zx + bz}$.

Exem. 4. Sit denique ordinata applicata $\frac{bx^{\frac{3}{2}}}{\sqrt{5c^2 - 3accz^2 + 3aacz^3 - a^2xz}}$.
 Hæc, ad formam regulæ reducta, fit $bz^{\frac{3}{2}} \times c - az^{\frac{3}{2}}$. Indeque est $d = b$;
 $\theta = \frac{1}{2}$; $e = c$; $f = -a$; $\eta = \frac{3}{2}$; $\lambda = -\frac{1}{2}$; $r = 2$; $s = \frac{7}{2}$; $Q = -\frac{3b}{2a} \times c - az^{\frac{3}{2}}$;

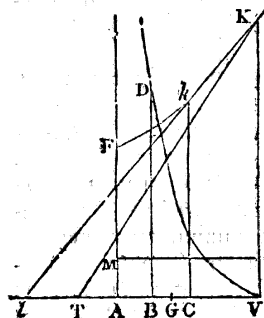
$\pi = \frac{1}{2}$. Et area $Q \times \frac{5z^{\frac{3}{2}}}{7} - \frac{1}{2} \times \frac{5c}{7a}$, id est $-\frac{30abz^{\frac{3}{2}} + 75bc}{28aa} \times c - az^{\frac{3}{2}}$.

Quòd si res non successisset in hoc casu, existente r vel fractione vel nu-
 mero negativo: tunc tentassem alterum casum, purgando terminum
 $-az^{\frac{3}{2}}$ in ordinatam applicatam à coefficiente $z^{\frac{3}{2}}$; hoc est reducendo ordina-
 tam applicatam ad hanc formam; $bz^{-\frac{1}{2}} \times \frac{1}{a + cz^{-2}}$. Et si r in neu-
 tro casu fuisset numerus integer & affirmativus; conclusissem Curvam ex
 earum numero esse, quæ non possunt geometricè quadrari. Nam, quan-
 tum animadverto, hæc regula exhibet in finitis æquationibus areas omnium
 geometricarum quadraturarum admittentium, Curvarum; quarum ordinatam
 applicatæ constant ex potestatibus, radicibus, vel quibuscumque digunitatibus
 binomii cujuscunque, licet non directè, ubi index dignitatis est numerus
 integer.

At quando hujusmodi Curva aliqua non potest geometricè quadrari;
 sunt ad manus alia theorematum pro comparatione ejus cum conicis sectioni-
 bus, vel saltem cum aliis figuris simplicissimis, quibuscum potest comparari:
 ad quod sufficit etiam hoc ipsum unicum jam descriptum theorema, si de-
 bite concinnetur.

Pro

Pro trinomiis etiam, & aliis quibusdam*, regulas quasdam concinnavi. BURGUM
POSTERIOR.
 Sed in simplicioribus vulgoque celebratis figuris, vix aliquid relatu dig-
 num reperi, quod evasit aliorum conatus; nisi fortè Longitudo Cissoïdis
 ejusmodi censcatur. Ea sic constructur.



Sit vd cissoïdis; AV, diameter circuli ad quem
 aptatur; V, vertex; AF, asymptotos ejus; ac DB,
 perpendiculare quodvis ad AV demissum. Cum
 semi-axe AF = AV, & semi-parametro AG =
 1/2 AV, describatur hyperbolæ FKK; & inter AB
 & AV sumptâ AC mediâ proportionali, eri-
 gantur, ad C & V, perpendicula cK, vK, hyper-
 bolæ occurrentia in k & K; et agantur rectæ,
 KT, kt, tangentes hyperbolam in eisdem K &
 k, & occurrentes AV in T & t; & ad AV
 constituatur rectangulum VM æquale spacio
 TKkt. Et cissoïdis VD longitudo erit sextupla
 altitudinis AM. Demonstratio perbrevis est (*).

Sed ad infinitas series redeo.

Quamvis multa restent investiganda, circa modos approximandi, & circa
 diversa serierum genera, quæ possunt ad id conducere: tamen vix cum D.
 Tschurnhausio speraverim, dari posse aut simpliciora, aut magis generalia
 fundamenda reducendi quantitates ad hoc genus serierum, de quo agimus,
 quàm sunt divisiones, & extractiones radicum; quibus Leibnitiis & ego uti-
 mur; saltem non generaliora: quia pro quadraturâ & Εὐθύρει Curvarum
 ac similibus, nullæ possunt dari series ex hisce simplicibus terminis alge-
 braicis (unicam tantum indefinitam quantitatem involventibus) constantes,
 quas non licet hæc methodo colligere.

Nam non possunt esse plures convergentes series ad id determinandum,
 quàm sunt indefinitæ quantitates, ex quarum potestatibus series consen-
 tur: & ego quidem, ex adhibitâ quâcunque indefinitâ quantitate, seriem
 novi colligere; & idem credo Leibnitio in potestate esse.

Nam quamvis meâ methodo liberum sit eligere, pro consilandâ serie,
 quantitatem quamlibet indefinitam, à quâ quæsitum dependeat; & metho-
 dus, quam ipse nobiscum communicavit, determinata videatur ad electio-
 nem talium indefinitarum quantitatum, quibus opus commodè deduci po-
 test ad fractiones, quæ per solam divisionem evadant series infinitæ: ta-
 men aliæ quæcunque indefinitæ quantitates pro seriebus consilandis adhi-
 beri possunt; per methodum istam, quâ affectæ æquationes resolvuntur;
 dummodo resolvantur in propriis terminis; hoc est, conficiendo seriem ex
 solis terminis, quos æquatio involvit.

Præterea non video, cur dicatur his divisionibus & extractionibus proble-
 mata resolvi per accidens: siquidem hæ operationes eodem modo se habeant
 ad hoc genus algebrae, ac vulgares operationes arithmeticae ad algebram
 vulgo notam.

* Hæ omnes regulæ propositionem quintam sextam septimam & octavam Libri de Quadraturis
 constituunt.

(*) Consule Geometriam Analyticam, Cap. XII, § 43, 44.

4 A 2

Quod

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AD OLDEN.

Quod autem ad simplicitatem methodi attinet; nolim fractiones & radicales, absque præviâ reductione, semper resolvi in series infinitas: sed, ubi perplexæ quantitates occurrant, tentandæ sunt omnimodæ reductiones; five fiat augendo, minuendo, multiplicando, vel dividendo quantitates indefinitas; five per methodum transmutatoriam Leibnitii; aut alio quocunque modo, qui occurrat. Et tunc resolutio in series, per divisionem & extractionem, opportunè adhibebitur.

Hic autem præcipue nitendum est, ut denominatores fractionum, & quantitates in vinculo radicum, reducantur ad quàm paucissimas & minimè compositas; & ad tales etiam, quæ in seriem abeunt citissime convergentem, etsi radices neque convertantur in fractiones, neque deprimantur. Nam, per regulam initio alterius epistolæ, extractio altissimarum radicum æquè simplex & facilis est, ac extractio radicis quadraticæ, vel divisio: & series, quæ per divisionem eliciuntur, solent minimè omnium convergere.

N^o LX.

Hactenus de seriebus unicam indefinitam quantitatem involventibus locutus sum. Sed possunt etiam, perspectâ methodo, series ex duabus, vel pluribus, assignatis indefinitis quantitatibus pro arbitrio confici. Quinetiam beneficio ejusdem methodi possunt series ad omnes figuras efformari, Gregorianis, ad circulum & hyperbolam editis, affines; hoc est, quarum ultimus terminus exhibebit quæsitam aream. Sed calculum hic onerosiorem nolim lubens subire.

Possunt denique series ex terminis compositis eadem methodo constitui.

Quemadmodum, si sit $\sqrt{aa - ax + \frac{x^2}{a}}$ ordinatim applicata Curvæ alicujus;

pono $aa - ax = zz$; & ex binomio $zz + \frac{x^2}{a}$ extractâ radice, prodibit $z + \frac{x^2}{2az} - \frac{x^4}{8a^2az^3}$ &c. Cujus seriei omnes termini quadrari possunt per theorema jam antè descriptum. Sed hæc minoris facio; quod ubi series simplices non sunt satis tractabiles, aliam nondum communicatam methodum habeo, quâ pro lubitu acceditur ad quæsitum.

Ejus fundamentum est commodè, expeditè, generalis solutio hujus problematis: *Curvam geometricam describere, quæ per data quacunq; puncta transibit* (*).

Docuit Euclides descriptionem circuli per tria data puncta. Potest etiam conica sectio describi per quinque data puncta; & Curva trium dimensionum per septem data puncta: adeo ut in potestate habeam descriptionem omnium Curvarum istius ordinis, quæ per septem tantum puncta determinantur. Hæc statim geometricè sunt, nullo calculo interposito. Sed superius problema est alterius generis: & quàmvis primâ fronte intractabile videatur, tamen res aliter se habet. Est enim ferè ex pulcherrimis, quæ solvere desiderem.

Seriei à D. Leibnitio pro quadraturâ conicarum sectionum propositæ, affinia sunt theorematum quædam, quæ, pro comparatione Curvarum cum conicis sectionibus, in catalogum * dudum retuli.

(*) Vide Methodum Differentialem, Tom. I. p. 521—528.

* Ex his patet propositiones Newtoni de Quadraturâ Curvarum diu ante annum 1676 inventas fuisse.

Possum

Possum utique cum sectionibus conicis geometricè comparare Curvas omnes (numero infinitas infinitas) quarum ordinatim applicatæ sunt

BUREAU
POSTERIOR.

$$\frac{dz^{n-1}}{e+fz^2+gz^{2n}} \quad \text{vel} \quad \frac{dz^{2n-1}}{e+fz^2+gz^{2n}} \quad \&c.$$

$$\text{Aut} \frac{dz^{n-1}}{e+fz^2+gz^{2n}} \quad \text{vel} \quad \frac{dz^{2-1}}{e+fz^2+gz^{2n}} \quad \&c.$$

$$\text{Aut} \frac{d}{z} \sqrt{e+fz^2+gz^{2n}} \quad \text{vel} \quad dz^{n-1} \times \sqrt{e+fz^2+gz^{2n}} \quad \&c.$$

$$\text{Aut} \frac{dz^{n-1}}{\sqrt{e+fz^2+gz^{2n}}} \quad \text{vel} \quad \frac{dz^{2n-1}}{\sqrt{e+fz^2+gz^{2n}}} \quad \&c.$$

$$\text{Aut} \frac{dz^{n-1} \times \sqrt{e+fz^2}}{g+bz^2} \quad \text{vel} \quad \frac{dz^{2n-1} \times \sqrt{e+fz^2}}{g+bz^2} \quad \&c.$$

$$\text{Aut} \frac{dz^{n-1}}{g+bz^2 \times \sqrt{e+fz^2}} \quad \text{vel} \quad \frac{dz^{2n-1}}{g+bz^2 \times \sqrt{e+fz^2}} \quad \&c.$$

$$\text{Aut} \frac{d}{z} \sqrt{\frac{e+fz^2}{g+bz^2}} \quad \text{vel} \quad dz^{n-1} \times \sqrt{\frac{e+fz^2}{g+bz^2}} \quad \&c.$$

Hic d, e, f, g significant quasvis datas quantitates, cum suis signis + & - affectas; z , axem vel basem Curvæ; & $\eta, 2\eta, \frac{1}{2}\eta-1, \frac{1}{4}\eta-1, \eta-1, 2\eta-1$, indices potestatum vel dignitatum z , five sint affirmativi vel negativi, five integri vel fracti: & singula bina theorematum sunt duo primi termini seriei in infinitum progredientis. In tertio & quarto, g debet esse non majus quàm ff , nisi e & g sint contrarii signi. In cæteris nulla est limitatio. Horum aliqua (nempe, secundum, tertium, quartum, quintum, & decimum tertium) ex areis duarum conicarum sectionum conjunctis constant. Alia quædam (ut nonum, decimum, & duodecimum) sunt aliter satis composita. Et omnia quidem, in continuatione progressionum, citò evadunt compositissima; adeo ut vix per transmutationem figurarum, quibus Jacobus Gregorius & alii usi sunt, absque ulteriori fundamento inveniri possent.

Ego quidem haud quicquam generale in his obtinere potui, antequam abstraherem à contemplatione figurarum, & rem totam ad simplicem considerationem solarum ordinatim applicatarum reducerem. Sed, cum hæc, & hisce generaliora, sint in potestate; non dubitabitur, credo, de binomialibus longè facilioribus, quæ in his continentur; & prodeunt, ponendo literam aliquam, e vel f vel $g, = 0$; & $\eta = 1$ vel 2 , etsi series, in quas ista resolvantur, non posuerim in epistolâ priori, nedum fortè computaverim; intentus, non in omnia particularia enumeranda, sed in illustrandam methodum per unam & alteram, in singulis rerum generibus, instantiam; quæ ad ostendendam ejus generalitatem sufficere videbatur.

Cæterum hæc theorematum dant series plusquam uno modo. Nam pri^o LX. mum, si ponatur $f = 0$, & $\eta = 1$, evadit $\frac{d}{e+gz^2}$; unde prodit series nobis communicata. Sed si ponatur $zg = ff$, & $\eta = 1$, inde tandem obtinemus hanc

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NEWTONI
AD OLDEN. hanc seriem * $1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9} + \frac{1}{11} - \frac{1}{13} - \frac{1}{15}$ &c. pro longitudine qua-
drantalís arcús, cújus chorda est unitas: vel, quod perinde est, hanc
 $\frac{1}{2} + \frac{1}{15} - \frac{1}{63} + \frac{1}{143} - \frac{1}{355}$ &c. pro longitudine dimidii ejus. Et has fortè,
quia æquè simplices sunt ac alteræ, & magis convergunt, non repudiabitis.
Sed ego rem aliter æstimo. Illud enim melius quod utilius est, & pro-
blema minori labore solvit. Sic, quamvis hæc æquatio $x^2 - x = 1$ appareat
simplicior hæcce, $yy - 2y\sqrt{\frac{81}{25}} - \sqrt{20} = \sqrt{20}$; tamen in confesso est poste-
riorem reverà simpliciore[m] esse; propterea quòd radicem ejus, y , geometra
faciliùs eruit.

Et ob hanc rationem series pro obtinendis arcubus circuli, vel, quod eodem recidit, pro obtinendis sectoribus conicarum sectionum, pro optimis habeo, quæ componuntur ex potestatibus finium.

Nam si quis vellet per simplex computum hujus seriei $r + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9}$ &c. colligere longitudinem quadrantis ad viginti figurarum loca decimalia, opus esset 5 000 000 000 terminis seriei circiter; ad quorum calculum milleni anni requirerentur. Et res tardiùs obtineretur per tangentem 45 graduum. Sed, adhibito finu recto 45 graduum, quinquaginta quinque vel sexaginta termini hujus seriei, $\sqrt{\frac{1}{2} \times 1 + \frac{1}{12} + \frac{1}{160} + \frac{1}{896}}$ &c. sufficerent: quorum computatio tribus, ut opinor, vel quatuor diebus absolvi posset.

Et tamen hic non est optimus modus computandi totam peripheriam. Nam series ex sinu recto 30 graduum, vel sinu verso 60 graduum conflata, multo citius dabit arcum suum; cujus sextuplum, vel duodecuplum, est tota peripheria. Neque majori labore eruitur, area totius circuli, ex segmento, cujus sagitta est quadrans diametri. Ejus computi specimen, siquidem ad manus est, visum fuit apponere; & unâ adjungere aream hyperbolæ, quæ eodem calculo prodit.

Posito axe transverso = 1, & sinu verso, seu segmenti sagittæ, = x ; erit
 semi-segmentum hyperbolæ } = x^4 in $\frac{2}{3}x - \frac{x^2}{5} - \frac{x^3}{28} + \frac{x^4}{72}$ &c. Hæc autem
 circuli

series sic in infinitum producitur, fit $2x^2=a$; $\frac{ax}{2}=b$; $\frac{bx}{4}=c$; $\frac{3cx}{6}=d$; $\frac{5dx}{8}=e$; $\frac{7ex}{10}=f$; &c. Et erit semi-segmentum hyperbolæ } $=\frac{a+b}{3-5}$
circuli }

$\frac{c}{7} - \frac{d}{9} - \frac{e}{11} + \frac{f}{13}$ &c. Eorumque semi-fumma $\frac{a}{3} - \frac{c}{7} - \frac{e}{11} -$ &c. & semi-
 differentia $\frac{b}{5} + \frac{d}{9} + \frac{f}{13} +$ &c. His ita preparatis; suppono $x = \frac{1}{4}$, qua-

drantem nempe axis; & prodit $a (= \frac{1}{4}) = 0.25$; $b (= \frac{ax}{4} = \frac{0.25}{1 \times 8}) = 0.03125$; $c (= \frac{bx}{4} = \frac{0.03125}{2 \times 8}) = 0.001953125$; $d (= \frac{cx}{6} = \frac{0.001953125}{8}) = 0.000244140625$. Et sic procedo, usque dum venero ad terminum depressissimum, qui potest ingredi opus. Deinde hos terminos per 3, 5, 7, 9, 11, &c.

* D. Vicecomes Broucker hyperbolam per hanc seriem $\frac{1}{1 \times 2} + \frac{1}{3 \times 4} + \frac{1}{5 \times 6} + \frac{1}{7 \times 8} + \dots$ id est

E P I S T O L I C U M.

&c. respectivè divisos, dispono in duas tabulas: ambiguos cum primo in DUCUM
unam; & negativos in aliam; & addo, ut hâc vides, POSTERIOR.

0.08	3333333333333333	
6250000000000000		0.0002790178571429
271267361111		34679066051
5135169396		834465027
144628917		26285354
4954581		961256
190948		38676
7963		1663
352		75
16		4
		<hr/>
		0.0002825719389575

0.0896109885646618

Tunc à priori summâ aufero posteriorem; & restat 0.0893284166257043
area semi-segmenti hyperbolici. Addo etiam eas summas, & aggregatum
aufero à primo termino duplicato, 0.1666666666666666; & restat
0.0767731061630473 area semi-segmenti circularis. Huic addo
triangulum istud, quod completur in sectorem, hoc est $\frac{1}{2}r\sqrt{3}$, seu
0.0541265877365274; & habeo sectorem 60° graduum,
0.130896938995747; cujus sextuplum, 0.7853981033974482, est area
totius circuli: quæ divisa per $\frac{1}{2}$, five quadrantem diametri, dat totam peri-
pheriam 3.1415926535897928. Si alias artes adhibuissim; potui, per eun-
dem numerum terminorum seriei, pervenisse ad multo plura loca figurarum,
puta viginti quinque, aut amplius: sed animus fuit hic ostendere, quid, per
simplex seriei computum, præstari possit. Quod sane haud difficile est;
cum in omni opere multiplicatores ac divisores, magnâ ex parte, non majores
quàm 12, & nunquam majores quàm 41, adhibere opus sit.

Per seriem Leibnitii etiam, si ultimo loco dimidium termini adjiciatur, & alia quedam finitima artificia adhibeantur, potest computum produci ad multas figuras. Ut & ponendo summam terminorum $1 - \frac{1}{7} + \frac{1}{9} - \frac{1}{13} + \frac{1}{15} - \frac{1}{17} + \frac{1}{19} - \frac{1}{21} + \frac{1}{23}$ &c. esse ad totam seriem $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} +$ &c. ut $1 + \sqrt{2}$. ad 2. Sed optimus ejus usus videtur esse, quando vel conjungitur eam duabus aliis perlimilibus, & citissime convergentibus, seriibus; vel sola adhibetur ad computandum arcum 30 graduum, posita tangente $\sqrt{\frac{1}{3}}$. Tunc enim series illa evadit $1 - \frac{1}{3 \times 3} + \frac{1}{5 \times 9} - \frac{1}{7 \times 27} + \frac{1}{9 \times 81}$ &c. quæ cito convergit. Vel, si conjunges cum aliis seriibus, pone circuli diametrum = 1, & $a = \frac{1}{3}$; & aëa totius circuli erit summa harum trium serierum $\frac{a}{1} - \frac{a^3}{3} + \frac{a^5}{5} - \frac{a^7}{7} + \frac{a^9}{9} - \frac{a^{11}}{11} +$ &c. $\frac{a^2}{1} - \frac{a^4}{3} + \frac{a^6}{5} - \frac{a^8}{7} + \frac{a^{10}}{9} - \frac{a^{12}}{11} +$ &c. $\frac{a^4}{1} - \frac{a^6}{3} + \frac{a^8}{5} - \frac{a^{10}}{7} + \frac{a^{12}}{9} - \frac{a^{14}}{11} +$ &c.

est per hanc $1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \frac{1}{5} - \frac{1}{6} + \frac{1}{7} - \frac{1}{8} + \&c.$ (conjunctis binis terminis) primus omnium quod dravit. Mercator hanc quadraturam aliter demonstravit. Gregorius communicavit hanc seriem pro circulo $1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \frac{1}{9} - \frac{1}{11} + \&c.$ & Newtonus hanc $1 + \frac{1}{3} - \frac{1}{5} + \frac{1}{7} + \frac{1}{11} - \frac{1}{13} - \frac{1}{17} + \&c.$

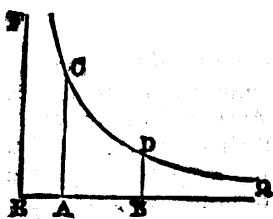
NEWTON
AD OLDEN

Hic consideravimus series, quatenus adhibentur ad computandum totum circum. Sed quando computandæ sunt partes ejus, tunc quælibet series habet proprium usum, & in suo genere optima est. Si datur tangens satis parva, vel satis magna; non recurrendum erit ad sinum aliquem ut inde computetur arcus, neque vice versa. Series, dato congruens, est æquatio pro solvendo proprio problemate.

Nº LXI.

Credo Cl. Leibnitium, dum posuit seriem pro determinatione cosinus ex arcu dato, vix animadvertisse seriem meam pro determinatione sinus versi ex eodem arcu; siquidem hæc idem sunt.

Neque observasse videtur morem meum generaliter usurpandi literas, pro quantitibus cum signis suis, + & -, affectis, dum dividit hanc seriem $\frac{z}{b} + \frac{zx}{2abb} + \frac{z^2}{6aab^2} + \frac{z^3}{24a^3b^3} + \&c.$ Nam cum area hyperbolica AD, hinc signifi-



cata per z , sit affirmativa vel negativa, prout jaceat ex unâ vel alterâ parte ordinatim applicatæ AC; si area illa in numeris data sit l , & l substituatur in serie pro z , orietur vel $\frac{l}{b} + \frac{\frac{ll}{2abb}}{24a^3b^3} + \frac{l^2}{6aab^2} + \frac{l^3}{24a^3b^3} + \&c.$ vel $-\frac{l}{b} + \frac{\frac{ll}{2abb}}{6aab^2} - \frac{l^2}{24a^3b^3} + \frac{l^3}{24a^3b^3} + \&c.$ prout l sit affirmativa vel negativa.

Hoc est posito $a=1=b$, & l logarithmo hy-

perbolico; numerus, ei correspondens, erit $1 + \frac{l}{1} + \frac{ll}{2} + \frac{l^2}{6} + \frac{l^3}{24} + \&c.$ si l sit affirmativus; & $1 - \frac{l}{1} + \frac{ll}{2} - \frac{l^2}{6} + \frac{l^3}{24} + \&c.$ si l sit negativus. Hoc modo fugio multiplicationem theorematum, quæ aliâs in nimiam molem crescerent. Nam v. g. illud unicum theorema, quod supra posui pro quadraturâ Curvarum; resolvendum esset in 32 theoremata, si pro signorum varietate multiplicaretur.

Præterea, quæ habet Vir Clarissimus de inventionem numeri, unitate majoris, per datum logarithmum hyperbolicum, ope seriei $\frac{l}{1} - \frac{ll}{1 \times 2} + \frac{l^2}{1 \times 2 \times 3} -$

$\frac{l^3}{1 \times 2 \times 3 \times 4} + \&c.$ potius quàm ope seriei $\frac{l}{1} + \frac{ll}{1 \times 2} + \frac{l^2}{1 \times 2 \times 3} + \frac{l^3}{1 \times 2 \times 3 \times 4} + \&c.$ nondum percipio. Nam si unus terminus adjiciatur amplius ad seriem posteriorem, quàm ad priorem, posterior magis appropinquabit. Et certè minor est labor, computare unam, vel duas primas figuras adjecti hujus termini, quàm dividere unitatem per numerum prodeuntem ex logarithmo hyperbolico, ad multa figurarum loca extensum, ut inde habeatur numerus quesitus unitate major. Utraque igitur series, si duas dicere fas sit, officio suo fungatur. Potest tamen $\frac{l}{1} + \frac{ll}{1 \times 2} + \frac{l^2}{1 \times 2 \times 3} + \frac{l^3}{1 \times 2 \times 3 \times 4} + \&c.$ series, ex dimidiâ parte terminorum constans, optimè adhiberi; siquidem hæc dabit semi-differentiam duorum numerorum, ex quâ, & rectangulo dato, uterque datur. Sic & ex serie $1 + \frac{ll}{1 \times 2} + \frac{l^2}{1 \times 2 \times 3} + \frac{l^3}{1 \times 2 \times 3 \times 4} + \&c.$ datur semi-summa numero-

rum,

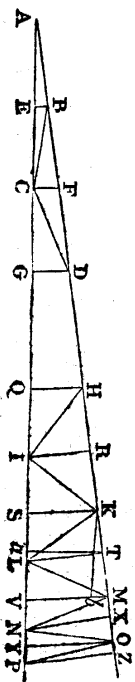
rum, indeque etiam numeri. Unde prodit relatio serierum inter se, quâ, ex unâ datâ, dabitur altera.

Theorema de inventionem arcus ex dato cosinu, ponendo radium 1, cosinum c , & arcum $\sqrt{6 - \sqrt{24c + 12}}$, minus appropinquat, quàm primâ fronte videtur. Posito quidem sinu verso v , error erit $\frac{v^3}{90} + \frac{v^5}{194} + \&c.$ Potest fieri ut $120 - 27v$ ad $120 - 17v$, ita chorda $(\sqrt{2v})$ ad arcum; & error erit tantum $\frac{61v^3 \sqrt{2v}}{44800}$ circiter; qui semper minor est quàm $5\frac{1}{4}$ minuta secunda, dum arcus non sit major quàm 45 grad. Et singulis etiam bisectionibus diminuitur 128 vicibus.

Series $\frac{a^2}{1 \times 2 \times 3} - \frac{a^4}{1 \times 2 \times 3 \times 4 \times 5} + \frac{a^6}{1 \times 2 \times 3 \times 4 \times 5 \times 6 \times 7} + \&c.$ applicari posset ad computationem tabulæ segmentorum, ut observat Vir Clarissimus. Sed res optimè absolvitur per canonem sinuum. Utpote, cognitâ quadrantis areâ, per continuam additionem nonæ partis ejus, habebis sectores ad singulos decem gradus in semicirculo; deinde, per continuam additionem decimæ partis hujus, habebis sectores ad gradus; & sic ad decimas partes graduum, & ultra, procedi potest. Tunc, radio existente 1, ab unoquoque sectore, & ejus complemento ad 180 gradus, aufer dimidium communis sinus recti, & relinquuntur segmenta in tabulam referenda. Cæterum quamvis series hinc non profint, in aliis tamen locum obtinent. Et quoniam hoc ad earum usum spectat, non gravabor in aliquibus attingere.

Constructionem logarithmorum non aliunde peti debere, credetis forte Nº LXII. ex hoc simplici processu, qui ab istis pendet. Per methodum supra traditam quærantur logarithmi hyperbolici numerorum 10, 0.98, 0.99, 1.01, 1.02: id quod fit spatio unius & alterius horæ. Dein, divisis logarithmis quatuor posteriorum per logarithmum numeri 10, & addito indice 2, prodibunt veri logarithmi numerorum 89, 99, 100, 101, 102, in tabulam referendi. Hi per dena intervalla interpolandi sunt; & exhibunt logarithmi omnium numerorum inter 980 & 1000: & omnibus inter 980 & 1000 iterum per dena intervalla interpolatis, habebitur tabula eatenus constructa. Tunc ex his colligendi erunt logarithmi omnium primorum numerorum, & eorum multiplicium, minorum quàm 100: ad quod nihil requiritur, præter additionem & subtractionem: Siquidem fit $\sqrt[10]{\frac{9984 \times 1020}{9945}} = 2.$ $\sqrt[8]{\frac{8 \times 9963}{984}} = 3.$ $\frac{10}{2} = 5.$ $\sqrt{\frac{98}{2}} = 7.$ $\frac{99}{9} = 11.$ $\frac{1001}{7 \times 11} = 13.$ $\frac{102}{6} = 17.$ $\frac{988}{4 \times 13} = 19.$ $\frac{9736}{16 \times 27} = 23.$ $\frac{986}{2 \times 17} = 29.$ $\frac{992}{32} = 31.$ $\frac{999}{27} = 37.$ $\frac{984}{24} = 41.$ $\frac{989}{23} = 43.$ $\frac{987}{21} = 47.$ $\frac{9911}{11 \times 17} = 53.$ $\frac{9971}{13 \times 13} = 59.$ $\frac{9882}{2 \times 81} = 61.$ $\frac{9949}{3 \times 49} = 67.$ $\frac{994}{14} = 71.$ $\frac{9928}{8 \times 17} = 73.$ $\frac{9954}{7 \times 18} = 79.$ $\frac{996}{12} = 83.$ $\frac{9968}{7 \times 16} = 98.$ $\frac{9894}{6 \times 17} = 97.$ Et habitis sic logarithmis omnium numerorum minorum quàm 100; restat tantum hos etiam, semel atque iterum, per dena intervalla interpolare.

C O M M E R C I U M



Construētiois tabulæ finium, à quâ pendet tota res trigonometrica, fundamentum optimum est continua additio dati anguli ad seipsum, vel ad alium datum. Utpote in angulo addendo BAE; inscribantur HI, IK, KL, LM, MN, NO, OP, &c. æquales radio AB: & ad opposita latera demittantur perpendiculares, BE, HQ, IR, KS, LT, MV, NX, OY, &c. Et angularum HIQ, IKH, KLI, LMK, &c. differentię erunt angulus A; sinus HQ, IR, KS, &c.; & cosinus IQ, KL, LS, &c. Detur jam aliquis eorum LMK, & cæteri sic eruentur. Ad sv & mv demitte perpendiculara, ta & kb; & (propter similia triangula ABE, TLa, Km̄b, ALt, AMv, &c.) erit AB.BE:: TL.La ($= \frac{SL-LV}{2}$): : KT ($= \frac{1}{2}KM$). $\frac{1}{2}Mb$ ($= \frac{MV-KS}{2}$). Et

$AB.AE :: KT.sa (= \frac{SL+LV}{2}) :: TL.Ta (= \frac{KS+MV}{2})$. Unde
 dantur sinus & cosinus KS, MV, SL, LV . Et simul patet ratio
 continuandae progressionis. Nempe $AB.2AE :: LV.TM + MX$
 $:: MX.VN + NY \&c. :: MV.TL + XN :: XN.MV + OY \&c.$
 Vel $AB.2BE :: LV.XN - TL :: MV.TM - MX :: MX.OY - MV ::$
 $XN.VN - NY \&c.$ Et retro, $AB.2AE :: LS.KT + RK \&c.$ Pone
 ergo $AB = 1$; & fac $BE \times TL = La$; $AE \times KT = sa$; $sa - La =$
 LV ; $2AE \times LV - TM = MX \&c.$ Sed nodus est inventio finis
 & cosinus anguli A . Et hinc subveniunt series nostrae. Ut-
 pote cognita, ex superioribus, quadrantalibus arcus longitudine
 $1.57079 \&c.$ & simul quadrato ejus, $2.4694 \&c.$ divide
 quadratum hoc per quadratum numeri exprimentis rationem

90 graduum ad angulum A: & quoto dicto x, tres vel quatuor termini
hujus seriei, $1 - \frac{x}{2} + \frac{x^2}{24} - \frac{x^3}{720} + \frac{x^4}{40320}$ &c. dabunt cosinum istius anguli A.

Sic primò quæri potest angulus 5 graduum, & inde tabula computari ad quinos gradus; ac deinde interpolari ad gradus, vel dimidios gradus, per eandem methodum. Nam non convenit progredi per nimios saltus. Dux tertię partes tabulę, sic computatę, dant reliquam tertiam partem per additionem vel subtractionem, more noto. Siquidem posito $\kappa\tau$ cosinus 60 graduum; sit $AE = SV$, & $BE = Mb$. Tunc ad decimas & centesimas partes graduum pergendum est per aliam methodum; substitutis tamen prius logarithmis sinuum inventorum, si ejus generis tabula desideretur.

Ad computum tabularum astronometricarum Kepleri; & posui fundamentum aliquod in alterâ epistolâ. Ejus seriei tres primi termini, & aliquando duo, sufficiunt. Sed ad diversas partes ellipseos diversæ ejusmodi series aptari debent. Vel potius tales series computandæ sunt, quæ ex datâ areâ sectoris elliptici BGE, immediate exhibeant aream sectoris circuli, cujus angulus est BEG, radius CB. Et habitis hisce, computum earum ad duos, tres, aut forte quatuor terminos, beneficio logarithmorum, haud gravius erit, quam solita resolutio tot triangulorum in aliis hypothesebus: imo fortè minus grave, si series prius debite concinnentur; siquidem unus logarithmus, è tabulâ

tabulâ petitus, determinet omnes istos terminos, addendo ipsum, & ejus mul-
tiplices, ad logarithmos datarum coefficientium in promptu habitos.

Quæ de hoc genere tabularum dicuntur, ad alias transferri possunt, ubi ratiocinia geometrica locum non obtinent. Sufficit autem per has series computare triginta, vel viginti, aut fortè pauciores terminos tabulæ in debitis distantis; siquidem termini intermedii facile interferuntur per methodum quandam, quam in usum calculatorum ferè hîc descripsiſſem. Sed pergo ad alia.

Quæ Cl. Leibnitiuss à me desiderat explicanda, ex parte suprà descripsi. N^o LXIII. Quod verò attinet ad inventionem terminorum p, q, r , in extractione radicis affectæ: primum, p , sic eruo. Descripto angulo recto BAC, latera ejus BA, CA dividò in partes æquales; & inde normales erigo, distribuentes angulare spatium in æqualia parallelogramma vel quadrata; quæ concipio de-

B **Fig. 1.**

x^4	x^4y	x^4yy	x^4y^3	x^4y^4	x^4y^5	x^4y^6
x^3	x^3y	x^3yy	x^3y^3	x^3y^4	x^3y^5	x^3y^6
x^2	x^2y	x^2yy	x^2y^3	x^2y^4	x^2y^5	x^2y^6
x	xy	xyy	xy^3	xy^4	xy^5	xy^6
0	y	yy	y^3	y^4	y^5	y^6

Fig. 2.

nominata esse à dimensionibus duarum indefinitarum specierum, puta x & y , regulariter ascendentium à termino A ; prout vides in fig. 1. inscriptas. Ubi y denotat radicem extrahendam; & x alteram indefinitam quantitatem, ex cujus potestatibus series conficienda est. Deinde, cum æquatio aliqua proponitur, parallelogramma, singulis ejus terminis correspondentia, insignio notâ aliquâ: & regulâ ad duo, vel fortè plura, ex insignitis parallelogrammis applicatâ (quorum unum sit humillimum in columna sinistrâ juxtâ AB , & alia ad regulam dextrorsum sita, cæterasque omnia non contingentia regulam supra eam jaceant) seligo terminos æquationis per parallelogramma, contingentia regulam, designatos; & inde quæro quantitatem quotienti addendam.

Sic ad extrahendam radicem y , ex $y^6 - 5xy^5 + \frac{x^2}{2}y^4 - 7a^2x^3y^3 + 6a^2x^4 + b^2x^6 = 0$; parallelogramma, hujus terminis respondentia, signo notā aliquā *; ut vides fig 2. Dein applico regulam, DE, ad inferiorem ē locis signatis in finistrā columnā; eamque ab inferioribus ad superiora dextrorum gyrare facio, donec alium similiter, vel fortē plura, ē reliquis signatis locis cooperit attingere. Videoque loca sic attacta esse x^1 , xyx & y^6 . E terminis itaque $y^6 - 7a^2xyx + 6a^2x^1$ tanquam nihilo æqualibus (& insuper si placet reductis ad $v^6 - 7vvv + 6 = 0$, ponendo $y = v\sqrt{ax}$) quæro valorem y , & invenio quadruplicem, $+\sqrt{ax}$, $-\sqrt{ax}$, $+\sqrt{2ax}$, & $-\sqrt{2ax}$: quorum quælibet pro primo termino quotientis accipere licet, prout ē radicibus quampiam extrahere decretum est.

NEWTONI
AD OLDER- Sic * æquatio $y^3 + axy + aay - x^3 - 2a^3 = 0$, quam resolvebam in priori epistolâ, dat $-2a^3 + aay + y^3 = 0$; & inde $y = a$ proximè. Cum itaque a sit primus terminus valoris y , pono p pro cæteris omnibus in infinitum, & substituo $a + p = y$. (Obvenient hîc aliquando difficultates nonnullæ; sed ex iis, credo, D. Leibnuitius se proprio Marte extricabit.) Subsequentes verò termini, q, r, s , &c. eodem modo ex æquationibus secundis, tertiis, cæterisque eruantur, quo primus p è primâ; sed curâ leviori; quia cæteri valores y solent prodire, dividendo terminum involventem infimam potestatem indefinitæ quantitatis, x , per coefficientem radicis p, q, r aut s .

Nº LXIV. Intellexiti credo, ex superioribus, regressionem, ab areis Curvarum ad lineas rectas, fieri per hanc extractionem radicis affectæ. Sed duo alii sunt modo, quibus idem perficio.

Eorum unus affinis est computationibus, quibus colligebam approximationes sub finem alterius epistolæ, & intelligi potest per hoc exemplum. Proponatur æquatio ad aream hyperbolæ $z = x + \frac{1}{2}xx + \frac{1}{2}x^3 + \frac{1}{4}x^4 + \frac{1}{5}x^5$ &c. Et partibus ejus multiplicatis in se, emerget $z^2 = x^2 + x^3 + \frac{1}{12}x^4 + \frac{5}{6}x^5$ &c. $z^3 = x^3 + \frac{3}{2}x^4 + \frac{7}{4}x^5$ &c. $z^4 = x^4 + 2x^5$ &c. $z^5 = x^5$ &c. Jam de z aufero $\frac{1}{2}z^2$, & restat $z - \frac{1}{2}z^2 = x - \frac{1}{6}x^3 - \frac{5}{24}x^4 - \frac{1}{30}x^5$ &c. Huic addo $\frac{1}{6}z^3$, & fit $z - \frac{1}{2}z^2 + \frac{1}{6}z^3 = x + \frac{1}{24}x^4 + \frac{3}{40}x^5$ &c. Aufero $\frac{1}{24}z^4$, & restat $z - \frac{1}{2}z^2 + \frac{1}{6}z^3 - \frac{1}{24}z^4 = x - \frac{1}{120}x^5$ &c. Addo $\frac{1}{120}z^5$, & fit $z - \frac{1}{2}z^2 + \frac{1}{6}z^3 - \frac{1}{24}z^4 + \frac{1}{120}z^5 = x$ quamproximè; five $x = z - \frac{1}{2}z^2 + \frac{1}{6}z^3 - \frac{1}{24}z^4 + \frac{1}{120}z^5$ &c.

Eodem modo series de unâ indefinitâ quantitate in aliam transferri possunt. Quemadmodum si posito r radio circuli, x sinu recto arcûs z , & $x + \frac{x^3}{6rr} + \frac{3x^5}{40r^4} + \&c.$ longitudine arcûs istius; hanc seriem à sinu recto ad tangentem vellem transferre: quæro longitudinem tangentis $\frac{rx}{\sqrt{rr - xx}}$, & reduco in infinitam seriem $x + \frac{x^3}{2rr} + \frac{3x^5}{8 \times 4} + \&c.$ Vocetur hæc quantitas t . Colligo potestates ejus; $t' = x^3 + \frac{3x^5}{2rr} + \&c.$; $t'' = x^5 + \&c.$ Aufero

autem t de z ; & ponendo 1 pro r , restat $z - t = -\frac{x^3}{3} - \frac{3x^5}{10}$ &c. Adde $\frac{1}{2}t^2$, & fit $z - t + \frac{1}{2}t^2 = \frac{1}{2}x^2 + \text{&c.}$ Aufero $\frac{1}{2}t^2$, & restat $z - t + \frac{1}{2}t^2 - \frac{1}{2}t^2 = 0$ quamproximè. Quare est $z = t - \frac{1}{2}t^2 + \frac{1}{2}t^2 - \text{&c.}$ Sed siquis in usus trigonometricos me jussisset exhibere expressionem arcus per tangentem; eam, non hoc circuitu, sed directâ methodo quævissem.

Per hoc genus computi colliguntur etiam series, ex duabus, vel pluribus, indefinitis quantitativis constantes; & radices affectarum æquationum magnâ ex parte extrahuntur. Sed ad hunc posteriorem usum, adhibeo potius methodum in alterâ epistolâ descriptam, tanquam generaliorem, & regulis

* Hanc Resolutionem, vid. N° VII.

† Id est, *Una methodus consistit in extractione fluentis quantitatis, ex æquatione simul involvente fluxi-
um ejus: altera tantum in assumptione feriei, pro quantitate quolibet incognitâ, ex quâ cætera commodè deri-
vari possunt, & in collatione terminorum homologorum æquationis residuantiæ, ad erundenos terminos assumptæ
feriei.* Analyfin inverfam, per fluentes & earum momenta, in æquationibus tam infinitis quam fi-
nitis, Newtonus in his epistolis ad regulas quatuor reduxit. Per primam extrahitur fluens ex bi-
nomiis,

gulis pro elisione superfluum terminorum habitis, paulo magis expectatam.

Pro regressione verò ab arcibus ad lineas rectas, & similibus, possunt hujusmodi theorematum adhiberi.

Theorema 1. Sit $z = ay + byy + cy^3 + dy^4 + ey^5$ &c. Et vicin erit $y =$
 $\frac{z}{a} - \frac{b}{a^3} z^2 + \frac{2bb-ac}{a^5} z^3 + \frac{5abc-5b^3-aad}{a^7} z^4 + \frac{3aac-21abb+6aab+14b^4-a^2e}{a^9} z^5$ &c.

Exempli gratiâ. Proponatur æquatio ad aream hyperbolæ, $z = y - \frac{y^2}{2} +$

$\frac{y^3}{3} - \frac{y^4}{4} + \frac{y^5}{5} \&c.$ Et substitutis in regulâ 1 pro a , $-\frac{1}{2}$ pro b , $\frac{1}{3}$ pro c , $-\frac{1}{4}$

pro d , & $\frac{1}{5}$ pro e ; vicissim exurgit, $y = z + \frac{1}{2}zx + \frac{1}{6}z^2 + \frac{1}{24}z^3 + \&c.$

Theor. 2. Sit $x = ay + by' + cy'' + dy''' + ey^{(4)} + \&c.$ Et vicissim erit $y =$
 $\frac{x}{a} - \frac{b}{a^2}x^2 + \frac{3bb - ac}{a^3}x^3 + \frac{8abc - aad - 12b^3}{a^4}x^4 + \frac{55b^4 - 55abb^2 + 10aabd + 5aaac - a^2e}{a^5}x^5 \&c.$

Exempli gratiâ. Proponatur æquatio ad arcum circuli, $z = y + \frac{y^3}{6rr} + \frac{3y^5}{40r^4} +$

$\frac{5y^7}{112r^6}$ &c. Et substitutis in regulâ 1 pro a , $\frac{1}{6rr}$ pro b , $\frac{3}{4or^4}$ pro c , $\frac{5}{112r^6}$ pro d ,

&c; orietur $y = z - \frac{z^3}{6rr} + \frac{z^5}{120r^4} - \frac{z^7}{5040r^6} + \&c.$

Alterum modum regrediendi ab areis ad lineas rectas celare statui.

Ubi dixi, omnia pene problemata solubilia existere; volui de iis præsertim intelligi, circa quæ Mathematici se hactenus occupârunt, vel saltem in quibus ratiocinia mathematica locum aliquem obtinere possunt. Nam alia sane, adeo perplexis conditionibus implicata, excogitare liceat, ut non satis comprehendere valeamus; & multo minus tantarum computationum onus sustinere, quod ista requirerent.

Attamen, ne nimium dixisse videar, inversa de tangentibus problemata. sunt in potestate; aliaque illis difficiliora. Ad quæ solvenda usus fuit duplici methodo; unâ concinniori, alterâ generaliiori. Utramque visum est impræsentia literis transpositis consignare; ne, propter alios idem obtinentes, infutitum in aliquibus mutare cogere. + 5accdaæioeffb12i4/3m1on6oqqr7si1i1ov3x: 11ab3cddi1oægi1oill4m7n6o3p3q6r5fi1i7vx, 3acc4egb6i4i4-m5n8oq4r3/6t4v, aaddæeeeeeiimnnnooprrrrsssstuu.

Inversum hoc problema de tangentibus, quando tangens inter punctum contactus & axem figuræ est datæ longitudinis, non indiget his methodis. Est tamen Curva illa mechanica, cujus determinatio pendet ab areâ hyperbolæ.

Ejusdem generis est etiam problema, quando pars axis, inter tangentem & ordinatim applicatam, datur longitudine.

nomiis, adeoque ex æquationibus quibuscunque non affectis, in serie infinitâ; & momentum fluentis simul prodit, quo evanescente, series in æquationem finitam redit. Per secundam, extrahitur fluens ex æquationibus affectis fluxionem non involventibus. Per tertiam, extrahitur fluens ex æquationibus affectis fluxionem simul involventibus. Per quartam, eruitur fluens ex conditionibus problematis. Regulæ duæ primæ in principio epistolæ superioris, duæ ultimæ in fine hujus ponuntur. Harum regularum Newtonum esse inventorem primum, nemo dubitat.

Sed

Sed hos casus vix numeraverim inter ludos naturæ. Nam quando in triangulo rectangulo, quod ab illâ axis parte, & tangente, ac ordinatim applicatâ constituitur, relatio duorum quorumlibet laterum, per æquationem quamlibet, definitur; problema solvi potest absque meâ methodo generali: sed ubi pars axis, ad punctum aliquod positione datum terminata, ingreditur vinculum; tunc res aliter se habere solet.

Communicatio resolutionis affectarum æquationum, per methodum Leibnitii, pergrata erit; juxta & explicatio quomodo se gerat, ubi indices potestatum sunt fractiones; ut in hac æquatione: $20 + x^{\frac{1}{2}} - x^{\frac{3}{2}} - y^{\frac{1}{2}} = 0$, aut surdæ * quantitates; ut in hac, $\sqrt{x^2 + x\sqrt{y}}^{\frac{1}{2}} = y$: ubi $\sqrt{2}$ & $\sqrt{7}$ non designant coefficientes ipsius x , sed indices potestatum, seu dignitatum ejus; & $\sqrt{\frac{1}{2}}$ indicem dignitatis binomii $x^{\frac{1}{2}} + x^{\frac{1}{2}}$. Res, credo, meâ methodo patet; aliter descripsissem.

Sed meta tandem prolixæ huic epistolæ ponenda est. Literæ sane excellentissimi Leibnitii valdè dignæ erant, quibus fusi hoc responsum darem. Et volui hac vice copiosior esse, quia credidi amœniora tua negotia, severiori hoc scribendi genere, non debere à me crebro interpellari.

Nº LXV. *Excerpta ex epistolâ D. Collins ad D. Newtonum, Londini, 5 Martii 1677 datâ. Integra autem extat impressa in tomo tertio Operum D. Wallisii pag. 646, &c.*

Clarissime Vir,

Aderat hîc D. Leibnitiu per unam septimanam, in mense Octobris; in reditu suo ad ducem Hanoveræ; cujus literis revocatus erat, in ordine ad quandam promotionem.

Dixit Leibnitiu, se posse & velle consilia impertire, pro obtinendis feriis, absque speciosâ extractione radicum æquationum affectarum; modò quis velit laborem illum obire.

Et consequenter ad hoc, postquam ego D. Bakerum ipsi nominaveram, literis ejus ad D. Oldenburgium, datis Amstelodami, 11 Novemb. 1676, hæc scribit.

* D. Collinio hæc quæso communica. Dixit ille mihi D. Bakerum, doctum admodum & industrium apud vos analyticum, utilibus consiliis exequendis parem esse. Elegi ego unum præ reliquis utile & facile. Nimirum, methodus tangentium, à Slusio publicata, nondum rei fastigium tenet. Potest aliquid amplius præstari in eo genere; quod maximi foret usus ad omnis generis problemata: etiam ad meam (sine extractionibus) æquationum ad series reductionem. Nimirum, posset brevis quædam calculi circa tangentes tabula, eoque continuanda, donec progressio tabulæ apparet; ut eam scilicet quisque, quousque libuerit, sine calculo continuare possit.

* Surdos indices D. Leibnitiu in epistolâ sequente mutavit in fluentes; & inde natus est calculus exponentialis.

* Amstelodami

* Amstelodami cum Huddenio locutus sum; cui negotia civilia tempus omne eripiunt. Est enim ex numero duodecim urbis Consul, qui subinde imperium obtinent: nuper Consul regens erat; nunc Thesaurarii munus exercet. Præclara admodum in ejus ichedis superesse, certum est. Methodus tangentium, à Slusio publicata, dudum illi fuit nota. Amplior ejus methodus est, quàm quæ à Slusio fuit publicata. Sed & quadratura hyperbolæ Mercatoris ipsi, jam anno 1662, innotuit. Hactenus Leibnitiu.

P. S. Exemplar epistolæ tuæ, quatuor schedarum, nondum est ad D. Leibnitiu missum: sed, intra septimanam, est quidam hinc profecturus Hanoveram, qui tum illud, tum libros quosdam laturus est.

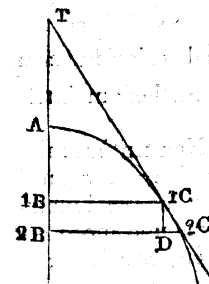
Epistola D. Leibnitii ad D. Oldenburgum, 21 Junii 1677, data cum D. Nº LXVI. Newtono communicanda. Cujus extat & autographum, & exemplar manu D. Collins descriptum.

Amplissime Domine,

Accepi literas tuas diu expectatas, cum inclusis Newtonianis sane pulcherrimis; quas plus semel legam cum curâ & meditatione; quibus certè non minus dignæ sunt, quàm indigent. Nunc pauca, quæ festinante oculo obeanti incidere, è vestigio annotabo.

Egregiè placet, quòd descripsit, quâ viâ in nonnulla sua elegantia sane theoremata inciderit. Et quæ de Wallisianis interpolationibus habet, vel ideo placent, quia hæc ratione obtinetur harum interpolationum demonstratio; cum res ea antea (quod sciam) solâ inductione niteretur; tametsi pars eorum per tangentes sit demonstrata.

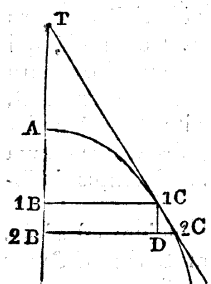
Clarissimi Slusii methodum tangentium nondum esse absolutam, celeberrimo Newtono assentior. Et jam à multo tempore + rem tangentium longè generalius tractavi; scilicet per differentias ordinarum. Nempe TIB intervallum tangentis ab ordinatâ in axe sumptum, est ad 1B1C ordinatam; ut 1CD, differentia duarum abscissarum A1B, A2B, ad 22C, differentiam duarum ordinarum 1B1C, 2B2C. Nec refert, quem angulum faciunt ordinatæ ad axem. Unde patet, nihil aliud esse invenire tangentes, quàm invenire differentias ordinarum, positis differentiis abscissarum (seu 1B2B = 1CD) si placet æqualibus. Hinc nominando † in posterum dy differentiam duarum proximarum y (nempe A1B & A2B); & dx, seu 22C, differentiam duarum proximarum x (prioris 1B1C, posterioris 2B2C); patet dy esse 2ydy; & dy esse 3y²dy, &c. & ita porro. Nam sint duæ proximæ



+ Idem fecit D. Barrow in ejus Lect. 10, anno 1669 impressâ, idque calculo consimili.

† Cœpit igitur D. Leibnitiu, hoc ipso tempore, methodum differentialem cum amicis scriptis communicare; lectis prius, quæ Newtonus de hac methodo in duabus epistolis scripserat; lectione fortè & aliis Newtonianis, sub finem anni 1676, ubi domum per Londinum redibat; quo tempore prælectiones Barrovii secum tulit.

libi



fibi (id est, differentiam habentes infinitè parvâ) scilicet $A1B = y$; & $A2B = y + dy$. Quoniam ponimus dy esse differentiam quadratorum ab his duobus rectis, æquatio erit $dy^2 = y^2 + 2ydy + dydy - y^2$. Seti omiſſis $y^2 - y^2$, quæ se destruunt; item omiſſo quadrato quantitatis infinitè parvæ, ob rationes ex methode de Maximis & Minimis notas; erit $dy^2 = 2ydy$. * Idemque est de cæteris potentiis. Hinc etiã haberi possunt differentiæ quantitarum, ex diversis indefinitis in se invicem ductis factarum: ut dyx erit $= ydx + xdy$; & $dy^2x = 2xydy + y^2dx$. Hinc si æquatio $a + by + cx + dyx + ey^2 + fx^2 + gy^2x + byx^2$ &c. $= 0$; statim habetur tangens Curvæ, ad quam est ista æquatio. Nam ponendo $AB = y$, & $A2B = y + dy$ (scilicet, quia $1B2B$ seu $1CD = dy$) itemque ponendo $1B1C = x$, & $2B2C = x + dx$ (scilicet quia $2CD = dx$): et, quia eadem æquatio exprimit quoque relationem inter $A2B$ & $2B2C$, quæ eam exprimebat inter $A1B$ & $1B1C$; + tunc in æquatione illâ pro y & x substituendo $y + dy$, & $x + dx$, fiet

$$\begin{array}{r} a+by+cx+dyx+ep^2+fx^2+gy^2+byx^2 \&c. \\ bdy+cdx+dydx+2eydy+2fxdx+2gxydy+2hxydx \&c. \\ +dxdy \quad +gy^2dx+hx^2dy \&c. \\ +ddxdy+edydy+fdxdx+gxdydy+bydxdx \\ d \text{ est quantitas communis more.} \quad +2gydydx+2hdxddy \&c. \\ d \text{ est nota differentia.} \quad +gdxdydy+bdydxdx \&c. \end{array} \quad \left. \vphantom{\begin{array}{l} a+by+cx+dyx+ep^2+fx^2+gy^2+byx^2 \&c. \\ bdy+cdx+dydx+2eydy+2fxdx+2gxydy+2hxydx \&c. \\ +dxdy \quad +gy^2dx+hx^2dy \&c. \\ +ddxdy+edydy+fdxdx+gxdydy+bydxdx \\ d \text{ est quantitas communis more.} \quad +2gydydx+2hdxddy \&c. \\ d \text{ est nota differentia.} \quad +gdxdydy+bdydxdx \&c. \end{array}} \right\} = 0.$$

d est quantitas communi more.

d est nota differentiae.

Ubi, abjectis illis quæ sunt supra primam lineam, quippe nihilo æqualibus per æquationem præcedentem; & abjectis illis quæ sunt infra secundam, quia in illis duæ infinitè parvæ in se invicem ducuntur; hinc restabit tantum æquatio hæc, $b dy + c dx + dy dx + dx dy = 0$; quicquid scilicet reperitur inter lineam primam & secundam. Et, mutatâ æquatione in rationem seu analogiam, fiet $-\frac{dy}{dx} = \frac{c + dy + 2fx + gy^2 + 2hxy \&c.}{b + dx + 2cy + 2gxy + bx^2 \&c.}$. Id est (quia $-\frac{dy}{dx}$ seu $\frac{-1B_2B}{D_2C}$, seu $\frac{-1CD}{D_2C} = -\frac{T_1B}{1B_2C}$) erit $\frac{c + dy \&c.}{b + dx \&c.} = -\frac{T_1B}{1B_1C}$. Quod coincidit cum regulâ Slusianâ; ostenditque eam statim occurrere hanc methodum intelligenti.

Sed methodus ipsa (prior) nostra longe est amplior. Non tantum enim exhiberi potest, cum plures sunt literæ indeterminatæ quàm y & x (quod sæpe fit maximo cum fructu); sed & tunc utilis est, cum interveniunt irrationales; quippe quæ eam nullo morantur modo: neque ullo modo necesse est irrationales tolli, quod in methodo Slusii necesse est, & calculi difficultatem in immensum auget.

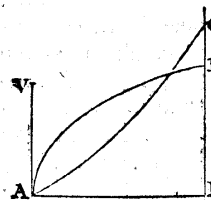
* Id est, si secundus terminus binomii sit differentia primi termini; secundus terminus potentiae binomii erit differentia potentiae. Hoc est fundamentum methodi differentialis à Leibnitio supra positum. Et hoc idem fundamentum methodi suae Newtonus anno 1669 posuerat in analysi supra impressa, N° XII. Per similibus calculis, Newtonus momenta, & Leibnitius differentias collegerunt; & discrepant solum in retin nominibus.

Quod ut appareat, tantum utile erit in irrationalitatibus simplicioribus rem explanare. Et primum fit in simplicissimis generaliter. Si fit aliqua potentia aut radix x^u ; erit $dx^u = ux^{u-1}dx$.

Si z fit $\frac{1}{2}$, seu si x^z fit \sqrt{x} , erit dx^z , seu hoc loco $d\sqrt{x} = \frac{1}{2}x^{-\frac{1}{2}}dx$ seu $\frac{dx}{2\sqrt{x}}$; ut notum, aut facilè demonstrabile.

Sit jam binomium; ut $\sqrt[3]{a+by+cy^2}$ &c. queritur $d\sqrt[3]{a+by+cy^2}$ &c. seu $dx^{\frac{1}{3}}$, posito $\frac{1}{3}=z$, & $a+by+cy^2$ &c. $=x$. Est autem $dx=b dy+2cy dy$ &c. Ergo $dx^{\frac{1}{3}}$, seu $\frac{dx}{3x^{\frac{2}{3}}} = \frac{b dy+2cy dy}{3 \times a+by+cy^2 \&c.}^{\frac{1}{3}}$. Eadem methodus adhiberi potest, et si radices in radicibus implicentur. Hinc si detur æquatio valde in-

cricata, ut $a + bx\sqrt{y^2 + b^2} : 1 + y + bx^2y\sqrt{y^2 + b^2} + y\sqrt{1 - y} = 0$, ad aliquam Curvam cujus absciffa fit y (AB) ordinata x (BC); tunc æquatio proveniens utilis ad inveniendam tangentem TC, statim sine calculo scribi poterit; & erit hæc $bdx\sqrt{y^2 + b^2} : 1 + y + \frac{bx}{2y\sqrt{y^2 + b^2} + 1 + y} \times 2ydy + \frac{bdy}{3 \times (1 + y)^{\frac{3}{2}}} + \frac{bx^2dy + 2bxydx \times \sqrt{y^2 + b^2} + y\sqrt{1 - y} + \frac{bya^2}{2y\sqrt{y^2 + b^2} + 1 + y} \times 2ydy + dy\sqrt{1 - y} - \frac{ydy}{2\sqrt{1 - y}} = 0$. Seu, mutando quotientem hanc inventam in analogiam, erit $-dy$ ad dx , seu TIB ad IBC, ut omnes provenientis æquationis termini, per dx multiplicati, ad omnes ejusdem terminos per dy multiplicatos.



† Calculus etiam, in his exemplis allatus, a calculo Newtoniano non differt: sed notis minus aptis obscurior redditur.

↑ Characteres methodi Newtoni Leibnitius hic enumerat; & gaudet, se in his methodis, quibusdam characteribus, quosdam characteres huiusmodi competunt. Fatetur etiam Newtonum, intellexisse facilem quadraturam figurarum, quibusdam characteribus huiusmodi competunt. Vel doceat, methodum aliam in rebus naturalibus, quæ sunt ad æquationem differentialem. Vel doceat, methodum Newtoni incidisse, in rebus naturalibus, quæ sunt ad æquationem differentialem. Vel doceat, methodum Newtoni incidisse, in rebus naturalibus, quæ sunt ad æquationem differentialem.

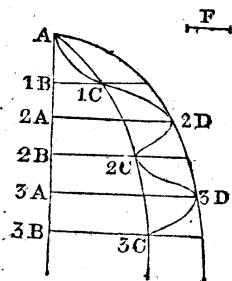
neralissimis quibusdam complecti. Licet nihil refert, siue series hæc producatur, siue ubilibet finiantur. Unde patet hanc unicam regulam pro infinitis figuris quadrandis inservire, diversæ plane naturæ ab iis, quæ hæcenus quadrari solebant.

Pulcherrimæ sunt illæ series Newtonianæ, quæ ex infinitis in finitas degenerant; qualis illa est, quam exhibet pro extractione radicum binomii, aut ejus quadraturâ. Quod si in ipsius generali illâ æquationis affectæ indefinitæ extractione, cum sit $x = ay + by^2 + cy^3$ &c. & y sit $\frac{x}{a} - \frac{bx^2}{a^2}$ &c. vel

$y = \frac{x}{a} - \frac{bx^2}{a^2}$ &c: idem præstari posset; ut scilicet, inter extrahendum radices ex æquationibus aut binomiis, invenire liceret radices rationales finitas, quando eæ insunt, vel etiam irrationales: tunc dicerem, methodum serierum infinitarum ad summam perfectionem esse perductam.

Opus esset tamen præterea, discerni posse varias æquationis ejusmodi radices: item necesse esset, ope serierum, discerni æquationes possibiles ab impossibilibus. Quod si hæc nobis obtinuerit vir in his studiis maximus, atque effecerit scilicet, ut possimus seriem infinitam convertere in finitam, quando id fieri potest, aut saltem agnoscere ex quânam finitâ sit deducta: tunc in methodo serierum infinitarum, quæ divisione & extractione inveniuntur, vix quicquam amplius optandum restabit. Hæc, si quisquam mortalium, certè Newtonus præstare poterit. Eadem credo operâ efficietur, ut, ex multis seriebus infinitis, possimus deligere maximè naturales; quales haud dubiè illæ erunt, quæ ita erunt comparatæ, ut, cum fieri potest, atque opus est, degenerent in finitas. Atque ita egregiè apparebit, methodum extractionum per series infinitas minimè indirectam, sed maximè naturalem esse.

Problema est perelegans, cujus meminit, Curvam describere, quæ per data quæcunque transeat puncta. Huddenius mihi Amstelodami dixit, posse se Curvam describere analyticam, seu certâ æquatione uniformi constantem, quæ faciei hominis cujusdam noti lineamenta designet.



Cæterum quærendum est, an hoc Newtonus intelligat de punctis infinitis; ut si sit axis AIB2A2B3A &c. in infinitum productus; & duæ Curvæ datæ infinitæ analyticæ, una A1C2C3C &c. altera A2D3D &c; si ponamus A1B, 1B2A, 2A2B, 2B3A, &c. inter se & datæ cuidam quantitati F æquales: quæritur, an dari possit Curva analytica, seu æquationis capax, quæ, in infinitum producta, transeat (alternis) per puncta 1C, 2D, 2C, 3D, 3C, &c. Fermatius alicubi scribit, se methodum habere, per quam Curva inveniri possit, cujus proprietas specifica data non

pertineat

* Vid pag. 519, lin. 36; & pag. 522, lin. 14 & seq.

† Dixerat Newtonus, analysin, beneficio æquationum infinitarum, ad omnia pene problemata sese extendere (pag. 530, lin. 24.) Respondit Leibnitijs, multa esse problemata usque adeo dura & implexa, ut neque ab æquationibus pendeant, neque à quadraturis; qualia sunt problemata methodi tangentium inversæ, &c. pag. 538, 539. Rescripsit Newtonus, inversa de tangentibus problemata esse in potestate, aliisque illis difficiliora; ad quæ solvenda se usum esse duplici methodo, &c. pag.

pertineat ad unum punctum, ut vulgo fit; cum ordinatæ referuntur ad partes axis; sed ad duo quælibet simul, vel etiam ad tria quælibet simul, &c.

* Quæ de variis seriebus suis & nostris examinandis, atque inter se comparandis, dicit Clarissimus Newtonus; in ea me immergere non audeo, atqueam in gratiam cum analysi rediero: nam harum rerum vestigia in animo meo prope non obliterata sunt. Agnosco interim, pulcherrimæ & utilissima ab eo annotari. Elegantissima & minimè expectata est via, quæ seriem meam $\frac{1}{2}t - \frac{1}{2}t^2 + \frac{1}{2}t^3$ &c. deduxit ex sua.

Quod ait, problemata methodi tangentium inversæ, esse in potestate; hoc arbitror ab eo intelligi per series scilicet infinitas. + Sed à me ita desiderantur, ut Curvæ exhibeantur geometricè quatenus id fieri potest, suppositis (minimum) quadraturis. Exempli causâ. Cycloïdem deprehendit Hugenus sui ipsius evolutione describi: difficile autem fuisset, credo,olvere hoc problema, invenire Curvam, quæ sui ipsius evolutione describitur. Neque refert, quod curvæ descriptio quadraturam circuli supponit: et hoc problema etiam ex eorum est numero, quæ voco methodi tangentium inversæ. Ita inter methodos tangentium inversas generales est, invenire Curvam analyticam, cujus longitudines sint areis datæ figuræ, curvæ analytica comprehensæ, proportionales. Contrarium enim dudum possumus. Quod problema arbitror non esse insolubile, & videtur non contemnendum: facilius enim est lineam, quàm spatium, organicè metiri. Et, reductâ spatiorum dimensione ad dimensionem linearum, solis filis in rectum extensis mechanica fieri poterit constructio; & spatia poterunt in datâ ratione secari, instar linearum rectarum.

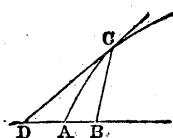
Cum ait Newtonus, investigationem Curvæ, quando tangens, vel inter-^{Nº LXVIII.}vallum tangentis & ordinatæ in axe sumptum, est recta constans, non indigere his methodis: innuit credo, se intelligere methodum tangentium inversam generalem in potestate esse, per methodos serierum appropinquativas; in hoc vero casu speciali † non opus esse seriebus. Ego vero methodum quærebam, quæ accuratè Curvam quæsitam exhibeat, saltem ex suppositis quadraturis; & cujus ope ejus æquationem, si quam habet, aut aliam primariam proprietatem possumus invenire.

Quod ait, problemata, in quibus datur relatio inter duo latera trianguli duc, semper posse solvi: id verum est; at ex § meis quoque artibus fluit; ac

557. Leibnitijs vero, ne quid à Newtono jam didicisse videretur, regebat solutionem à Newtono intelligi per series infinitas; sed à se ita desiderari, ut Curvæ exhibeantur geometricæ, quatenus id fieri potest. In priore epistola negaverat analysin Newtonianam, per æquationes infinitas, ad hæc problemata extendi. Jam negat se negasse; & verbis prioribus nubem obducit, quasi inversum illud problema suo sensu non solveretur, nisi Curvæ exhibeantur geometricè quatenus id fieri potest, & Curva, quæ sui ipsius evolutione describitur, inveniri possit per eandem solutionem.

† Hoc non dixit Newtonus, sed perspicue dixit problema in hoc casu non indigere methodis duabus generalibus, quas literis transpositis celaverat. Vide pag. 557.

§ Per artes suas intelligit methodum differentialem; ut patet ex calculis, quæ subjungit. Ubi epistolam priorem scribebat, problema de Curvâ invenendâ, in quâ intervallum tangentis & ordi-



ac sæpe, ne quadraturis quidem accitis, simplici analytica æquatione præstari potest. Ut, si BC positâ x , sit $DB = bx + cx^2 + dx^3$; quæritur, qualifnam sit hæc Curva, quæ hanc tangentium habeat proprietatem: id est, quænam sit æquatio relationem exprimens inter AB seu y , & BC seu x . Aio eam fore $y = bx + \frac{1}{2}cx^2 + \frac{1}{3}dx^3$. Si fuisset $DB = a + bx + cx^2$, opus fuisset quadraturâ hyperbolæ ad inveniendam Curvam quæsitam. Generaliter autem quomodocunque datur relatio inter duos ex lateribus trianguli (quod ego *Characteristicum*, ob crebros usus, vocare soleo) semper, suppositis quadraturis figurarum analyticarum, haberi potest Curva quæsitâ. Quod tamen nescio, an præter Newtonum præstiturus sit quisquam.

Meâ methodo, res unius lineolæ calculo peragitur, ac demonstratur. Sed & rem infinitis casibus præstare possum, tametsi ipsa y , seu AB, ingrediatur in ipsius DB expressionem. Ut, si sit $DB = bx + cx^2 + dx^3 + y$, fiet æquatio Curvæ $yx = bx + \frac{1}{2}cx^2 + \frac{1}{3}dx^3$. [Forte legendum, $DB = b + cx + dx^2 - y$, fiet æquatio Curvæ, $yx = bx + \frac{1}{2}cx^2 + \frac{1}{3}dx^3$.] Itaque si habeatur valor ipsius DA, ex BC haberi poterit Curva.

Quod verò ait Cl. Newtonus *, non æquè rem procedere, si detur relatio ipsius DB ad partem axis, seu ad AB vel y , ad hoc respondeo; mihi æquè facile esse, invenire Curvæ naturam vel æquationem, si detur relatio ipsius DB ad AB, quàm si, ut ipse requirit, detur relatio ad BC. Generalem verò methodum tangentium inversam nondum, quod sciam, habemus.

N^o LXIX. Sunt & alia problematum genera, quæ hæcenus in potestate non habeo; quorum ecce exempla. Sint duæ æquationes $x^m + y^m = xy$, & $x^m - y^m = x + y$. Duæ sunt incognitæ x , y , duæque ad eas inveniendas æquationes; quæritur valor tam unius quàm alterius literæ. Talia problemata vel in numeris vel in lineis solvere, difficillimum arbitror; si tamen de appropinquationibus agatur, puto posse iis satisfieri. Si quam huic difficultati lucem afferre potest Newtonus, pro eâ quæ pollet ingenii vi, multum analysim promovebit.

Analysis quoque Diophantæa, seu solutio problematum in numeris rationalibus, nondum perfectionem nacta est.

Hæc annotavi festinans, atque inter legendum; ad reliqua majore otio opus est: interea celeberrimum Newtonum quæso officiosissimè à me salutata, & post actas maximas gratias, eum roga, ut communicet continuationem harum serierum; nempe positâ $z = ay + by^2 + cy^3 + dy^4$ &c. ait fore $y = \frac{z}{a} - \frac{bz^2}{a^2} + \frac{2b^2 - ac}{a^3}z^3$ &c. vel $y = \frac{z}{a} - \frac{bz^2}{a^2} + \frac{3b^2 - ac}{a^3}z^3$ &c. Et si qua alia

nata, in axe sumptum, sit recta constans, vocabat ludum naturæ; & ejusmodi problemata, mira & implexa, ab æquationibus pendere noluit. Respondebat Newtonus, hoc problema non esse ludum naturæ; sed ubi datur relatio quavis inter ordinatam, & tangentem, & intervallum utriusque, in axe sumptum, semper posse solvi, idque absque suâ methodo generali; nempe per fluxionum methodum simplicem, & quadraturam Curvarum. Jam rescribit Leibnitiuss, id verum esse, at ex ejus quoque artibus fieri (id est ejusmodi problemata ab æquationibus suis pendere) & triangulum hæc, quæ crebros usus, characteristicum vocat, quasi hæc ipsi dudum innotuissent. Hujusmodi problemata ab æquationibus non pendere, anno superiore scripsit: jam fluunt horum solutiones ex ejus artibus.

in promptu habet theoremata nonnihil generalia; quoniam ad calculum contrahendum plurimum serviunt: quod si eorum originem, sive demonstrationem, addet, tanto magis obligabit. Velim etiam nosse, an per extractiones in seriebus discernere possit æquationes possibiles ab impossibilibus. Nam si generalis ejusmodi extractio procederet, sequeretur nullam æquationem fore impossibilem: item quomodo inveniatur diversas ejusmodi æquationis radices, ita ut ex pluribus radicibus eam possit invenire, quam quærimus: item an tales habeat series, quarum ope, extrahendo, æquationis inveniuntur valores finiti, quando tales insunt æquatione: denique quid sentiat de resolutione æquationum, quales paulo ante posui; ut $x^m + y^m = xy$, & $x^m + y^m = x + y$; ubi scilicet incognita ingreditur in exponentem.

Oblitus eram dicere, pulchram mihi videri Cissoidis extensionem in rectam, quam Newtonus invenit, ex suppositâ quadraturâ hyperbolæ. Ego mihi videor, eodem modo etiam metiri posse Curvam hyperbolæ æquilatæ, sed nondum omnis; neque Curvam ellipsoideam, quantum memini.

Antequam finiam, adjiciam usum pulcherrimum serierum, qui imprimis Collinio nostro non erit ingratus. Scis magnam esse difficultatem circa extrahendas radices ex binomiis cubicis, quando eas ingreditur quantitas imaginaria, orta ex radice quadraticâ negativæ quantitatis; ut $\sqrt[3]{a + \sqrt{-bb}}$ = $M + \sqrt[3]{a - \sqrt{-bb}}$ = N : ubi utraque quantitas, M & N , est singulatim impossibilis; summa autem, ut alibi ostendi, † est quantitas possibilis & realis, æqualis cuidam quæsitæ z . Ut vero ea eximatur, & ut extrahatur radix, nempe ut inveniatur $\frac{1}{2}z + e\sqrt{-bb} = \sqrt[3]{a + \sqrt{-bb}}$; & $\frac{1}{2}z - e\sqrt{-bb} = \sqrt[3]{a - \sqrt{-bb}}$ (unde fit $\sqrt[3]{a + \sqrt{-bb}} + \sqrt[3]{a - \sqrt{-bb}} = z$) non potest adhiberi methodus Schotenii, geometriæ Cartesianæ subjecta; quia opus est ad eam, ut valor ipsius $\sqrt[3]{a + \sqrt{-bb}}$ exhibeatur saltem approximando, quod notis methodis impossibile est. Quis enim valorem ipsius $\sqrt{-bb}$ prope verum dabit? necesse est enim invenire $b\sqrt{-1}$; quis autem exprimat $\sqrt{-1}$ appropinquando? Scripsi olim Collinio, me remedium invenisse; quod etiam ad omnes gradus superiores valeat: id ecce hic uno verbo. Ex binomio $\sqrt[3]{a + \sqrt{-bb}}$ extraho radicem per seriem infinitam, sive per theorema Newtonianum, sive etiam more meo priore, instituendo calculum secundum naturam cujusque gradûs, cum scilicet nondum theorema generale abstraxissem: quæ radix ponatur esse $l + m\sqrt{-bb} + n + p\sqrt{-bb}$ &c. Extrahatur jam & radix ex binomio altero, $\sqrt[3]{a - \sqrt{-bb}}$: fiet illa $+l - m\sqrt{-bb} + n - p\sqrt{-bb}$ &c. ut facillè demonstrari potest ex calculo: ergo ‡ addendo hæc duo extracta, destruentur imaginariæ quantitates, &

bis, ac sæpe ne quadraturis quidem accitis, simplici analytica æquatione (differentiali scilicet) peraguntur.

* Dixerat Newtonus, quod ubi relatio duorum quorumlibet, laterum trianguli desinere per æquationem, problema solvi potest absque generali ejus methodo, quam literis transmissis celebravit; sed ubi pars axis, vel axis, ingrediatur vinculum, res aliter se habere solet; id est, indiget ejus methodo generali, præterquam in particularibus quibusdam. Leibnitiuss, ad particularia illa alludens, sibi æquè facile esse ait invenire Curvæ naturam, vel æquationem, in utroque casu; quibus verbis manifestum, est solutionem generalem ei nondum innotuisse.

† Summa est quantitas triplex possibilis, ideoque non nisi tripliciter exhiberi potest.

‡ Examinanda est hæc methodus.

fiet.

fiet $z = 2l + 2u$ &c. quæ sunt eæ seriei portiones, in quibus nulla reperitur imaginaria. Invento ergo valore ipsius z , quantum satis est propinquo, quemadmodum Schotenius postulat, reliqua methodo Schotenianâ, perinde ac in illis binomiorum extrahendorum generibus, transiguntur.

Junii 21, 1677.

Nº LXX.

Epistola D. Leibnitii ad D. Oldenburgum, 12 Julii 1677 datâ, cum D. Newtono communicanda. Hujus extat exemplar manu D. Collins descriptum, & impressa est à D. Wallisio pag. 652.

Amplissime Domine,

Nuperas meas credo acceperis; nunc istas maturè summitto, ne facilitate D. Newtoni abutatur. Rogaveram enim in prioribus, ut quædam suæ epistolæ loca explicaret; nempe quomodo invenisset theorematâ, quòd posito $z = ay + by^2 + cy^3$ &c. fit $y = \frac{z}{a} - \frac{bz^2}{a^2} + \frac{3b^2 - ac}{a^3} z^3$ &c. vel $y = \frac{z}{a} - \frac{b^3}{a^2} + \frac{3b^2 - ac}{a^3} z^3$ &c. Nunc verò, relectis ejus literis, video id facile non tantùm ex ejus extractionibus derivari, sed & alterâ illâ methodo, sub finem literarum ejus expositâ inveniri; quâ me quoque * aliquando usum in veteribus meis schedis reperio; sed cum in exemplo, quod fortè in manus meas sumferam, nihil prodissset elegans, solitâ impatentiâ eam porro adhibere neglexisse.

Difficultatem moveram, in præcedentibus literis, circa æquationes impossibiles; quarum radices possibiles videntur inveniri per series infinitas; necdum verò illa sublata est, & meretur res excuti diligentius. Illud tamen video, si in æquatione datâ, $z = ay + by^2 + cy^3$ &c. literæ z & y sint indeterminatæ, tunc æquationem semper esse possibilem; sed si z esset determinata, rursusque in ipsis a vel b &c. lateret æquatio, posset esse impossibilis, & tamen per seriem generalem aliqua prodire videretur radix possibilis; cujus difficultatis solutionem, re diligenter expensâ, reperiri posse arbitror; sed nunc in ista accuratius inquirere non licet. Meretur autem explicari, tum quomodo ex seriebus agnosci possit æquationes esse impossibiles (quantum id aliâs satis facillè invenitur) tum quomodo dignoscantur diversæ radices.

Præter ea quæ in superiore epistolâ notavi, scilicet methodum tangentium inversam & geometricam (saltem suppositis Curvarum analyticarum quadraturis)

* D. Leibnitius series plures reciprocas ante biennium ab Oldenburgo acceperat: methodum serierum reciprocarum anno superiore Newtonum rogaverat: hoc anno acceptam ægrè intellexerat: & intellectam se olim invenisse, ex chartis suis antiquis mox didicit: et quamvis series pro hyperbolâ & circulo ante annos plures haberet, & hæc methodus ex arcu daret sinum, ex logarithmo daret numerum, & serierum omnium exhiberet reciprocas; eandem tamen olim inventam neglexisse, ut inutilem. Sic methodum, quam diu desideraverat, rogaverat, acceperat, & ægrè intellexerat, vel primus, vel saltem proprio Marte scilicet invenit.

† Quod hic desideratur, Newtonus, in epistolâ suâ novissimâ, significavit se aliquâ ex parte invenisse;

quadraturis) & alia id genus, + deest nobis circa quadraturas, ut scire certo possimus, an non quadratura figuræ alicujus propositæ reducat ad quadraturam circuli aut hyperbolæ: nam pleræque figuræ, hætenus tractatæ, ope alterutrius quadrari potuerunt. Quòd si demonstrari potest (ut arbitrator) quasdam figuras non esse quadrabiles nec per circulum nec hyperbolam; restat, ut alias quasdam figuras primarias altiores constituamus, ad quarum quadraturam reducantur cæteræ omnes, quando id fieri potest. Hoc quamdiu non fit, hæremus; & sæpe per seriem infinitam particularem quærimus, quod ad circuli aut hyperbolæ, aut aliam notioris figuræ quadraturam reduci poterat. Crediderat Gregorius, dimensionem Curvarum hyperbolæ & ellipseos non pendere à quadraturâ circuli aut hyperbolæ; ego verò reperi aliquam speciem Curvæ hyperbolicæ, quam, ex datâ ipsius hyperbolæ quadraturâ, metiri possum: de cæteris nondum mihi liquet.

Hannoveræ 12 Julii 1677.

Nº LXXI.

Brevi postea, autumno scilicet anni 1677, mors Oldenburgi huic literarum commercio finem imposuit. Deinde anno 1682 Collins mortuus est, & Acta eruditorum Lipsiæ primum edita sunt: ejusque anni mense Februario prodiit D. Leibnitii Quadratura Arithmetica, circuli scilicet & hyperbolæ; quarum prior non differt à Gregorianâ toties dictâ, neque posterior ab eâ Vicecomitis Brounkeri, ante quatuordecim annos, in Philosophicis Transactionibus Nº XXXIV. pro mense Aprilis 1668, publicatâ. Non multo post, anno scilicet 1684, in iisdem actis Lipsicis pro mense Octobri, Calculi Differentialis elementa primum edidit D. Leibnitius literis G. G. L. designatus. Anno autem 1683 ad finem vergente, D. Newtonus propositiones principales earum, quæ in Philosophiæ Principiis Mathematicis habentur, Londinum misit, eademque cum Societate Regiâ mox communicatæ sunt; annique 1686 liber ille ad Societatem missus est ut imprimeretur, proximoque anno mense Martio lucem vidit: & exemplar ejus D. Nicolao Fatio datum est ut ad Leibnitium mitteretur. Deinde anno 1688 epitome ejus in Actis Lipsicis impressa est: quâ lectâ D. Leibnitius epistolam de lineis Opticis, sbediasma de resistentia Medii, & motu Projectilium gravium in Medio resistente, & tentamen de Motuum Cælestium causis composuit, & in Actis Lipsicis, incunte anno 1689, imprimi curavit; quasi ipse quoque præcipuas Newtoni de his rebus propositiones invenisset, idque diversâ methodo, quâ vias novas geometricas aperuisset; & librum Newtoni tamen nondum vidisset.

nisse; & quod invenerat, postea publicavit in libro de Quadraturâ Curvarum.

‡ Hæc licentiâ concessâ auctores quilibet inventis suis facillè privari possunt. Viderat Leibnitius epitomen libri in Actis Lipsicis. Per commercium epistolicum, quod cum viris doctis passim habebat, cognoscere potuit propositiones in libro illâ contentas. Si librum non vidisset, videre tamen debuisset, antequam suas, de iisdem rebus, in itinere scriptas compositiones publicaret. Dicunt aliqui, falsas esse Tentaminis Propositiones xi, xii & xv; & D. Leibnitium ab his, per calculum suum, deduxisse Propositiones xix & xx ejusdem tentaminis. Talis autem calculus ad propositiones prius inventas aptari quidem potuit, non autem inventorem constituere.

Anno

Nº LXXII. Anno autem 1695 Opera Mathematica celeberrimi Wallisii duobus tomis Oxonii prodire: & in Actis Eruditorum anni insequentis mense Junio, habetur libri Epitome; in quâ sequentia leguntur, pag. 257 & seq.

Newtonianis etiam seriebus jam in Anglicanâ editione expositis, adjicit quædam quæ David Gregorius Scotus Professor Oxoniensis, & Archibaldus Pitcarnius Medicinæ Lugduni Batavorum Professor, non abludentia attulerunt. Addit Cap. 95. Algebræ, pag. 389, apud exteros (ut verba ejus sonant) etiam Leibnitium & Tschurnhausium nonnihil ejusmodi præstitisse, & apud Britannos Jacobum Gregorium, & Nicolaum Mercatorem; sed quæ sunt, ut plurimum, non nisi casus particulares intra ambitum generalem regularum Newtoni. Calculo quoque differentiali Leibnitii affinem esse methodum fluxionum Newtoni, in Principiis Naturæ Mathematicis primum editam, tum utraq; esse antiquiorem Barrowii; & omnes Wallisianæ Arithmetica Infinitorum superstrui, quæ Cavalieri geometriam promovit, ut hic Archimedeam. Exhibet etiam methodum quandam Josephi Raphson pro infinitis seriebus, libello Londini 1690 edito, sub titulo Analyseos Equationum Universalis, comprehensam. Cæterum ipse Newtonus, non minus candore quàm præclaris in rem mathematicam meritis insignis, * publice & privatim agnovit, Leibnitium tum cum, interveniente celeberrimo viro Henrico Oldenburgo Bremensi, Societatis Regiæ Anglicanæ tunc Secretario, inter ipsos, ejusdem jam tum Societatis Socios, commercium intercederet; id est jam fere ante annos viginti & amplius; calculum suum differentialem, seriesque infinitas, & pro iis quoque methodos generales habuisse; quod Wallisius, in præfatione Operum factæ inter eos communicationis mentionem faciens, præterit; quoniam de eo fortasse non satis ipsi constabat. Cæterum differentiarum consideratio Leibnitiana, cujus mentionem facit Wallisius, ne quis scilicet, ut ipse ait, causaretur de calculo differentiali nihil ab ipso dictum fuisse, meditationes aperuit, quæ aliunde non æquè nascebantur. Est enim differentia analyticum quiddam, & calculi capax; & quod rei caput est, summæ reciprocam; eaque demum ratione factum est, ut calculus analyticus non minus in Geometriâ altiore, quàm Cartesius à suo calculo excluderat, quàm in ordinariâ, ab ipso tractatâ, procedat. Et quemadmodum Apollonius, & alii veteres, habebant quidem proprietates ordinarum pro lineis conicis & aliis, ex quibus formatae sunt postea æquationes à Cartesio; ita similiter lineæ, quas ipse Cartesius, quippe calculo suo intrahiabiles, à Geometriâ excluderat, Leibnitianâ primum methodo æquationibus finitis sunt expressæ, & sub leges analyseos redactæ; quâ ratione, omnes earum proprietates analytico jam calculo investigari possunt, prorsus ut in ordinariis. Et cum antea per viam figurarum, & imaginationibus, etiam præstantissimi geometræ faciliora tantum assequi in his potuerint, nunc, ope hujus calculi, non tantum priora illa primo velut obtutu patent, quæ

* Methodum differentialem Moutoni & Leibniti habuit anno 1673, & suam esse voluit: methodum aliam differentialem nondum habuit: series postea habuit, sed quas anno 1675 ab Oldenburgo accepit, ab aliis prius accipere potuisset. Methodum generalem perveniendi ad ejusmodi series anno proximo ab Oldenburgo petiit, à Newtono accepit, antea non habuit. Methodum quadræpadi radices in speciebus à Newtono simul accepit, quâ methodus ejus per transmutationem

tunc merito admirationi erant, sed & multo magis abstrusa deteguntur, ad quæ imaginatio non pertingit, in quo consistit potissimus calculi analytici usus. Cæterum ipsum celeberrimum Wallisium, quo est candore, non dubitamus etiam nostratum meditationibus. si sufficientem earum habuisset notitiam, locum ampliore in suo opere daturum fuisse. Sed ipse queritur, ultimâ Algebræ suæ paginâ, hæc nostra Eruditorum Acta, in quibus bona earum pars continetur, minus sibi fuisse visa: unde neque illa satis sibi cognita ait, quæ de Geometriâ Incomparabilium, vel Analyse Infinitorum, à Leibnitio data fuere, quæ libenter alioqui in suo quoque opere exhibiturus fuerit. Cæterum hæc occasione & de Nicolao Mercatore, quem Wallisius velut inter suos recensere videtur, notare volumus, Germanum fuisse, & ex Holsatiâ oriundum, etsi in Angliam habitatum concesserit; eumque primum fuisse, quantum constat, qui quadraturam publicè dederit per seriem infinitam, tametsi tunc quoque Newtonus in eadem, ipso inscio, incidisset, eaque multo longius produxisset.

Excerpta ex epistolâ D. J. Wallisii ad D. Leibnitium, Oxonii 1º Decemb. Nº LXXIII. 1696 datâ; quâ respondetur ad ea, quæ ex Actis Eruditorum modo descriptis.

Dum hæc scripturus eram; ostendit mihi nonnemo, hesternò die, Acta Lipsica, pro mense Junii præsentis anni 1696. Quorum eruditus editor dignatus est inibi amplam meorum Operum Mathematicorum, Oxonii editorum, mentionem facere. Quo nomine me ipsi obstrictum sentio, & gratias habeo.

Sed conqueri videtur, saltem subinsinuare, quòd, quum Newtoni methodos fusiùs exposuerim; de Leibnitianis parcius dixerim. At nolim ego te, quem magni aestimo, à me quoquo modo læsum iri. Sed gratulor potius, te, in tantâ nobilitate positum, ad res nostras mathematicas descendere voluisse. Et tantum abest, ut velim ego tibi quocunque modo iniquus esse, ut si qua ferat occasio, demerere malim.

Dum addit eruditus editor, illas me fortè præterisse, quòd de illis mihi non satis constiterit; id omnino verum est.

Dicam utique quod res est, neque enim fateri pudet: tuarum ego rerum nihil, quod memini, vidi quicquam, præter hæc duo. Quorum alterum, illud est, quod inter Londinensium Collectiones Philosophicas habetur sed absque demonstratione, ex Actis Lipsicis descriptum, de quadrato diametri ad aream Circuli ut 1 ad $\frac{1}{2} - \frac{1}{3} + \frac{1}{4} - \frac{1}{5} + \frac{1}{6} - \frac{1}{7} + \frac{1}{8} - \frac{1}{9} + \frac{1}{10} - \frac{1}{11} + \frac{1}{12} - \frac{1}{13} + \frac{1}{14} - \frac{1}{15} + \frac{1}{16} - \frac{1}{17} + \frac{1}{18} - \frac{1}{19} + \frac{1}{20} - \frac{1}{21} + \frac{1}{22} - \frac{1}{23} + \frac{1}{24} - \frac{1}{25} + \frac{1}{26} - \frac{1}{27} + \frac{1}{28} - \frac{1}{29} + \frac{1}{30} - \frac{1}{31} + \frac{1}{32} - \frac{1}{33} + \frac{1}{34} - \frac{1}{35} + \frac{1}{36} - \frac{1}{37} + \frac{1}{38} - \frac{1}{39} + \frac{1}{40} - \frac{1}{41} + \frac{1}{42} - \frac{1}{43} + \frac{1}{44} - \frac{1}{45} + \frac{1}{46} - \frac{1}{47} + \frac{1}{48} - \frac{1}{49} + \frac{1}{50} - \frac{1}{51} + \frac{1}{52} - \frac{1}{53} + \frac{1}{54} - \frac{1}{55} + \frac{1}{56} - 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Alterum est illud de Testudine Quadrabili; cujus ego, ut de tuo, mentionem facio in Algebræ meæ postremo folio. Præter hæc duo si plura noverim, non reticuissem.

Tuam Geometriam Incomparabilem, vel Analysin Infinitorum, quam ibidem à te memoratam dixi, ego nondum vidi; nec ejus quicquam vel de nomine ante inaudiveram, quam prout ibidem ad calcem Algebræ dictum est.

Neque Calculi Differentialis vel nomen audivisse me memini, nisi postquam utrumque volumen absolverant operæ, eratque præfationis præfigendæ postremum folium sub prelo, ejusque typos jam posuerant typographæ. Quippe tum me monuit amicus quidam harum rerum gnarus, qui peregrè fuerat, tum talem methodum in Belgio prædicari, tum illam cum Newtoni methodo fluxionum quasi coincidere. Quod fecit, ut, transmotis typis jam positis, id monitum interferuerim.

Sed antè monueram, Algebræ, Prop. xcv. pag. 389, quod solum potui, Leibnitium & Tschurnhausium talia meditados; sed quæ ego non videram, necdum vidi. Et sicubi fortè videram literas G. G. L.; nesciebam, quem illæ virum indicabant.

Extant, credo, plura in Actis Lipsicis; sed quæ ego non vidi: ut nec tu, credo, vidisti Brounkeri Quadraturam Hyperbolæ, quæ extat in Transactionibus Londinensibus. Mihi que condonari potest, hæc ætate, qui annum octogesimum superavi, si non omnia sciscitarer.

Noveram quidem jamdudum, & indicavi, de rebus hujusmodi nonnulla te meditatam esse; tibi que cum Newtono, mediante Oldenburgo, interfuisse literas quasdam tuas: sed, quas ego non vidi, nec scio quales fuerint: eratque Oldenburgus diu mortuus, ut non potuerim ab illo sciscitari. Rogabam quidem per literas Newtonum nostrum, ut si eas penes se haberet, earum mihi copiam faceret literarum; sed retulit ille, se non habere. Et quidem periisse credo flammis inopinato correptas, cum pluribus Newtoni scriptis meliori luce dignis: & nisi per me stetisset, perissent etiam Newtoni literæ. Eoque animo rogabam, ut tuas illas cum Newtoni literis junctim ederem. Idque etiam nunc, si ferat occasio, facturus fortè sum, modo mihi dignaberis earum copiam facere*.

Quod Henricus Oldenburgus fuerit Bremensis; & Nicolaus Mercator Holsatus, quod suggerit eruditus editor, omnino verum esse credo; saltem Anglos non fuisse satis novi; eosque propterea Germaniæ vestræ non invideo, adeoque non Nostrates dixi, sed "Apud Nos:" nec tamen ideo minus eos aut amavi, aut æstimavi. Nam mihi perinde est, quæ quis gente sit; *Tras Tyrisiæ foret, nullo discrimine*—modo sit vir bonus & bene meritus. Sed apud nos diu vixerant; & quicquid hæc in re fecerint, apud nos factum est.

Quæ fusiùs exposui, ut sentias quàm tibi non iniquus fuerim, aut parum candidus.

1675 cum Leibnitio communicasse; & præterea Leibnitium in Angliâ fuisse anno 1673. Collinius enim, ab anno 1670 series, à Newtono & Gregorio acceptas, rogatus non rogatus liberrimè nec sine jactantia comminavit, ut ex superioribus patet. Et Pellius, cui hæc series ignota non erat, cum Leibnitio de seriebus verba habuit.

Ex

Ex epistola D. Leibnitii ad Wallisium scriptâ, 1^o Martii, ineuntis N° LXXIV. anni 1697.

— Quoniam videris nonnulla, in Actis dicta, ita accepisse, quasi animi parum erga Germanos æqui accuseris, & quasi vicissim tua recensendo extenuentur: putavi non ingratum tibi fore, si epistolam dominis editoribus Actorum scriberem, cujus hic exemplum addo; quâ, si ipsis videretur, Actis iisdem insertâ, satisfieri tibi, scrupulis illis sublatis, possit. [Habetur in Actis Lipsicis pro mense Junio 1697.]

Ego qui te magni facio, & publicè professus sum, quantum meo judicio tibi debeat altior Geometriâ, æquissimum puto viris præclarè, non de suo tantum seculo, sed & posteritate meritis debitas gratias rependi. Ut autem animi mei certior esse possis, ecce verbo tenus transcripta, quæ ipse de tuis meritis geometricis dixi, Actorum Lipsiensium mense Junio 1686, pag. 298.

"Paucis dicam, quid potissimum insignibus nostri seculi Mathematicis in hoc Geometriæ genere, meâ sententiâ, debeatur.

"Primum Galilæus & Cavallerius involutissimas Cononis & Archimedæ artes detegere ceperunt.

"Sed Geometria Indivisibilium Cavallerii scientiæ renaſcentis non nisi infantia fuit. Majora subsidia attulerunt triumviri celebres; Fermatius, inventâ methodo de Maximis & Minimis; Cartesius, ostensâ ratione lineas geometriæ communis (transcendentes enim exclusit) exprimendi per æquationes: et P. Gregorius à S. Vincentio, multis præclaris inventis. Quibus egregiam Guldini regulam de Motu Centri Gravitatis addo.

"Sed & hi intra certos limites consistere; quos transgressi sunt Hugenius & Wallisius, geometriâ inclyti. Satis enim probabile est, Hugeniana Heuratio, & Wallisiana Nelio & Wrennio, qui primi Curvis æquales rectas demonstrare, pulcherrimorum inventorum occasionem dedisse. Quod tamen meritiſſimæ laudi inventionum nihil detrahit.

"Secuti sunt hos Jacobus Gregorius Scotus, & Isaacus Barroviuſ Anglus: qui præclaris in hoc genere theorematibus scientiam mirificè locupletarunt.

"Interea Nicolaus Mercator Holsatus, Mathematicus & ipse præstantissimus, & primus (quod sciam) quadraturam aliquam dedit per Seriem Infinitam.

"At idem inventum non suo tantum Marte assecutus est, sed & universalî quâdam ratione absolvit, profundissimi ingenii Geometra Isaacus Newtonus. Qui, si sua cogitata ederet, quæ illum adhuc premere intelligo, haud dubiè nobis novos aditūs ad magnâ scientiæ incrementa compendiaque aperiret."

* Eas tandem obtinuit D. Wallisius à schediasmati Collinii; alteras Newtoni olim acceperat ab Oldenburgo.

† Mercator quadraturam D. Brounkeri per divisionem Wallisianam tantum demonstravit, ut supra.

Quibus deinde nonnihil de iis addo, * quæ meâ operâ accessere; præfertim dum novo calculi genere effeci, ut etiam Algebram transcendentiâ a y subijciantur; & Curvas, quas Cartesius à geometriâ malè excluserat, suis quibusdam + æquationibus explicare docui; unde omnes earum proprietates certo calculi filo deduci possunt: exemplò Cycloidis, cui æquationem ibidem assigno, $y = \sqrt{2x - xx} + f \frac{dz}{\sqrt{2x - xx}}$. Ubi f significat summationem; & d , differentiationem; x , abscissam ex axe inde à vertice; & y , ordinatam normalem.

De te autem queri, nunquam mihi in mentem venit; quem faciliè apparet nostra, in Actis Lipsiensibus prodita, non satis vidisse.

Quæ inter Oldenburgum & me commutatæ sunt literæ, quibus aliqua accesserint à D. Newtono excellentis ingenii viro, variis itineribus & negotiis, ab hoc studiorum genere planè diversis, vel periere, ut alia multa; vel jacent in mole chartarum aliquando excutienda digerendaque, ubi à necessariis occupationibus vacatio erit; quam mihi tam subito quam vellem promittere non possum.

Nº LXXV.

Ex epistola Wallisii ad D. Leibnitium, Apr. 6, 1697.

Vir Nobilissime Celèberrimeque,

Literas tuas humanissimas, Martii $\frac{1}{2}$ Hannoveræ datas, accepi, & osculatus sum, Martii 13 stilo nostro 1697; hoc est, Apr. 10, stilo novo. Mihique gratulor, quòd nobilissimo viro ego meaque non displicuerint. Veniam interim exorare debeo, si locorum distantia fecerit, ut eruditissima tua scripta, & inventa, minus ego sciverim aut intellexerim, quam vellem; & quidem, quis sit ille tuus Calculus Differentialis, non satis mihi comperitum sit; nisi quòd mihi nuper nunciatum est, eum cum Newtoni Doctrinâ Fluxionum quasi coincidere.

Nec pudet me meam hâc in parte ignorantiam fateri; qui jam ab aliquot annis contentus fuerim, hâc ætate, lampadem tradere; aliisque permittere, ut promoveant ea quæ, siqua, ego non infelicitè detexerim.

Quòd literas scripseris, in mei gratiam, ad editores Actorum Lipsicorum, favori tuo debeo, & grates habeo.

Quis eorum ille sit, qui mea scripta recensuit in Actis Lipsicis pro mense Junii 1696, ego quidem non scio; sed ei gratias habeo. Neque enim est, cur ego ei succensere debeam, si non primo intuitu statim perspexerit omnia

* Leibnitius, recitando inventa nova mathematica, præmittit methodum fluxionum; quasi analysis tota infinitesimalis solâ suâ operâ accesserat.

+ Annon Newtonus hujusmodi æquationes prius invenit; qui docuit fluentem ex æquatione fluxionem involvente extrahere, & Curvas Mechanicas ad æquationes numero terminorum infinitas reduxit, pergendo ab hujusmodi æquationibus finitis? Annon totâ Fluxionum methodus inversa, ubi de Curvis agitur, pendeat ab hujusmodi æquationibus ad Curvas applicatis?

‡ Quasi Leibnitius hoc non advertisset anno 1677, ubi primum incidit in methodum Newtoni. Vide literas ejus suprà impressas, p. 561. Certè methodum Newtoni, ante annum 167; inventam fuisse, Leibnitius ex literis ejus intellexerat, sed in Actis Lipsicis hoc nunquam agnovit. Vide suprà p. 481, 544, 545. Sic & se ab Oldenburgo series Newtonianæ & Gregorianæ, ineunte anno

onania; quæ penitus rimanti occurrissent, aut etiam sint occursura. Sufficit enim instituto suo, ut summa quæque carpat, & magis obvia; lestoribus permittendo, si penitiora desiderent, apud authores indicatos quærere. Nolim autem existimes, quòd in gentem vestram minus æquo sim animo; nam secus est, &c.

Ubi dicitur, *Nicolaum Mercatorem primum esse qui quadraturam aliquam dedit per seriem infinitam*: vide annon mea talis sit, Ar. Infin. Pr. 191.

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Interim, quemadmodum & Vietæ & Cartesiana methodus Analyseos Speciosæ nomine venit; discrimina tamen nonnulla supersunt: ita fortasse & Newtoniana & Mea differunt in nonnullis.

Mihi consideratio differentiarum & summarum, in seriebus numerorum, * primam lucem affuderat, cum animadverterem differentias tangentibus, & summas quadraturis respondere. Vidi † mox differentias differentiarum in Geometriâ *Oculis* exprimi. Et notavi mirabilem analogiam relationis inter differentias & summas, cum relatione inter potentias & radices. Itaque judicavi, præter affectiones quantitatis hæcenus receptas y, y^2, y^3, y^4, y^5 , &c. vel generaliter y^e , sive $[p]y$, vel potentia ipsius y secundum exponentem e ; posse adhiberi novas differentiarum vel fluxionum affectiones, dy, d^2y (seu ddy) d^3y (seu $dddy$) imo utiliter etiam occurrit d^2y , & similiter generaliterque d^ny .

Hæc jam affectione admissa, ‡ vidi commodè per æquationes exprimi posse quantitates, quas à suâ Analyfi & Geometriâ excluderat Cartesius; & Curvas, quas ille non rectè vocat Mechanicas, hæc ratione calculo non minus subijci, quàm ab ipso in Geometriam receptas. Et, quemadmodum Veteres jam æquationes Curvarum Locales observaverant, sed Cartesius tamen utilem operam nobis navavit, dum eas calculo suo expressit: ita putavi, me non inutiliter facturum, si ostenderem methodum Curvas, ab ipso exclusas, similiter per æquationes exprimendi; quarum ope, omnia de iis certo calculi filo haberi possint.

Et licet fatear, quemadmodum rem ipsam, in æquationibus Curvarum localibus facilioribus, calculo Cartesii expressam, jam tenebant Veteres; ita rem ipsam, meis æquationibus differentialibus facilioribus expressam, non potuisse tibi, aliisque egregiis viris, esse ignotam: non ideo minus tamen puto, & Cartesium & me aliquid utile præstitisse. Nam antequam talia ad constantes quosdam characteres calculi analytici reducuntur, tantumque omnia vi mentis & imaginationis sunt peragenda, non licet in magis composita abditaque penetrare; quæ tamen, calculo semel constituto, lusus quidem jocisque videantur.

Unde jam mirum non est, § problemata quædam, post receptum calculum meum, soluta haberi, quæ antea vix sperabantur: ea præsertim quæ ad transitum pertinent à Geometriâ ad Naturam. Quoniam scilicet vulgaris geometria minus sufficit, quoties Infiniti involvitur consideratio; quam plerisque Naturæ operationibus inesse, consentaneum est, quo melius referat autorem suum.

* Fatetur hic Leibnizius, methodum tangentium per differentias primam lucem ipsi affudisse; id est, methodum, quam Fermatius, Gregorius, Barroviu coluere, Newtonus N° X. ad quantitatum augmenta momentanea generaliter applicuit. Hancce tangentium methodum Leibnizius, lectis Newtonianis, meditat, p. 523, 544, 558, 559: generalem reddit, p. 503: & Newtonianæ similem esse statim videt, p. 561, 564.

† Fermatius & Schootenius hoc ante viderunt, determinando punctum flexûs contrarii in Conchoide.

‡ Leibnizius hoc non vidit ante annum 1677. Scripsit enim anno 1676, inversa tangentium problemata, & alia multa, ab æquationibus non pendere. Rescripsit Newtonus, hujusmodi problemata in potestate esse, nempe per æquationes suas. Et tum demum Leibnizius, à Newtono admodum, hæc vidit. Vide p. 538, lin. penult.

Hugenius certe, || qui hæc studia haud dubiè profundissimè inspexerat, multisque modis auxerat, initio parvi faciebat calculum meum, nondum perspectâ ejus utilitate. Putabat enim, dudum nota sic tantum novè exprimi: prorsus quemadmodum Robervallius, & alii initio, Cartesii Curvarum calculos parvi faciebant. Sed mutavit postea Hugenius sententiam suam; cum videret quàm commodâ esset hæc exprimendi ratio, & quàm facile per eam res involutissimæ evolverentur. Itaque maximi eam à se fieri, aliquot ante obitum annis, non tantum in privatis ad me aliosque literis, sed publicè quoque est professus.

Cæterum Transcendentium appellationem, nequid à me præter rationem in phrasi geometricâ novari putes, sic accipio, ut transcendentem quantitates opponam ordinariis & algebraicis: et Algebraicas quidem, vel ordinarias, voco quantitates, quarum relatio ad datas exprimi potest algebraicè; id est, per æquationes certi gradûs; primi, secundi, & tertii, &c; quales quantitates Cartesius solas in suam Geometriam recipiebat: sed Transcendentes voco, quæ omnem gradum algebraicum transcendent. Has autem exprimimus, vel per valores infinitos, & in specie per series (neque enim ipsas series Transcendentes voco, sed quantitates ipsis exprimendas) vel per æquationes finitas; easque vel differentiales; ut cum ordinata cycloidis methodo meâ exprimitur per æquationem ** $y = f \frac{xdx}{\sqrt{ay-yq}}$; vel exponen-

tiales; ut cum incognita quædam x exprimitur per hanc æquationem $x^* + x = 1$. Et quidem transcendentium exponentialem pro perfectissimâ habeo; quippe quâ obtentâ, nihil ultra quærendum restare arbitror; quod secus est in cæteris.

Primus autem, ni fallor, etiam Exponentiales æquationes introduxi; cum ignota ingreditur exponentem. Et jam anno primo †† Actorum Lipsienſium, specimen dedi in exemplo quantitatis ordinariæ Transcendentia literæ, ut res fieret intelligibilior; nempe, si quæretur $x^* + x = 30$, patet $x = 3$ satisfacere; cum sit $3^* + 3 = 27 + 3 = 30$.

Ex epistola Wallisii ad Leibnitium, Julii 30, 1697.

N° LXXVII.

Optaverim ita, ut tibi vacet tuum Calculum Differentialem, & Newtono suam Fluxionum Methodum, justo ordine exponere; ut quid sit utrique

§ Mirum est, hæc à D. Leibnitio dici; qui ex literis & Principiis Newtoni intellexerat, methodum solvendi hujusmodi problemata Newtono, ante annum 1671, innotuisse; & ipsum primum, per hanc methodum, problemata tradidisse, quæ ad transitum pertinent à geometriâ ad naturam.

|| Hugenius literas, quæ inter Newtonum & Leibnitium mediante Oldenburgo intercesserant, nunquam vidit.

** Legendum $y = f \frac{xdx}{\sqrt{ax-xx}}$. Idem sic designari potest $y = \frac{xx}{\sqrt{ax-xx}}$; vel sic, $y = \frac{xx}{\sqrt{ax-xx}}$.

Et nota, quod ubi differentia referuntur ad summas, rectius dicuntur partes. Sunt enim non differentia summarum, sed partes; & nullam relationem habent ad summas, nisi quatenus sunt earum partes.

†† Imo anno 1677. Vide pag. 560, 564.

commune, & quid intersit discriminis, & utramque distinctius intelligamus*.

N^o LXXVIII. *In dissertatione D. Nicolai Fatii Duillierii, R. S. S. de investigatione geometricâ lineæ brevissimi descensus, &c. Londini, anno 1699 editâ, pag. 18, hæc habentur.*

“Newtonum primum, ac pluribus annis vetustissimum, hujus calculi inventorem, ipsâ rerum evidentia coactus, agnosco: à quo utrum quicquam mutuatus sit Leibnitius, secundus ejus inventor, malo eorum, quàm meum, sit judicium, quibus visæ fuerint Newtoni literæ, aliique ejusdem manuscripti codices.”

Et respondit D. Leibnitius in Actis Lipsiensibus mense Maii 1700.

“Certè cum elementa calculi mea edidi anno 1684, + ne constabat quidem mihi aliud de inventis ejus in hoc genere, quàm quod ipse olim significaverat in literis; posse se tangentes invenire, non sublatis irrationalibus: quod Hugenus quoque se posse mihi significavit postea, et si cæterorum istius calculi adhuc expers: sed majora multo consecutum Newtonum, viso demum libro Principiorum ejus, fatis intellexi. Calculum tamen differentiali tam similem ab eo exerceri, non antè didicimus, quàm cum non ita pridem magni geometræ Johannis Wallisii Operum volumina primum & secundum prodire, Hugeniusque, curiositati meæ favens, locum inde descriptum, ad Newtonum pertinentem, mihi maturè transmisit.”

Et post aliqua, de methodi hujus parte sublimiore verba faciens, addit: “Quam [methodum] ante dominum Newtonum & me nullus quod sciam geometra habuit; uti ante hunc maximi nominis geometram, NEMO, ipsemine publicè dato, se habere probavit: ante dominos Bernoullios & me nullus communicavit.”

D. Fatio autem replicationem suam ad editores Lipsienses, ut publicaretur, mittente, hi, quasi lites averfati, eandem Actis suis inserere recusarunt. Vide Act. Lips. Martii 1701, pag. 134.

* Ut Leibnitius differentiam methodorum exponat, iterum rogat Wallisius; sed frustra.

+ Constabat certè D. Leibnitio, jam ab anno 1677, Curvarum quadraturas faciliores reddi; & problemata tangentium inversa D. Newtoni methodis solvi; idque nonnunquam per quadraturas solas, nonnunquam per methodos generatiles. Confer literas ejus pag. 561, & seq. cum pag. 544, 545, 547, 558, ut & cum pag. 510, lin. 25, 26, & pag. 523, lin. 4, 7, 17.

† Compilatores Actorum, in scribendis librorum Breviariis, à censuris temerariis abstinere debent. Ex hac censurâ patet animus scriptoris in D. Newtonum.

§ Literis D. T. Tschurnhausius designatur.

|| Hæc Isagoge, & scholium propositionis ultimæ, scripta sunt ubi liber prodit; reliqua ex MS antiquo, manibus amicorum trito, impressa sunt.

Tandem ubi prodire Newtoni Libri de Numero Curvarum secundi generis, de N^o LXXIX. que quadraturâ figurarum, editores Actorum Lipsiensium, stylo Leibnitiano, synopsis libri prioris his verbis concluderunt. Vide Act. Lips. Januarii 1705.

Cæterum autor non attigit focos, vel umbilicos, Curvarum secundi generis; & multo minùs generum altiorum. Cum † ergo ea res abstrusior sit indaginis, & maximi tamen in hoc genere usûs, tum ad descriptiones, tum ad alias proprietates Curvarum, & doctrinæ hæc focorum ab illustrissimo D. § D. T. profundiùs sit versata; supplementum ejus pro his Curvis ab ipsius ingenio expectamus.

Dein libri alterius synopsis sequentem, si synopsis dici mereatur, eodem stylo subjunxerunt.

Ingeniosissimus deinde autor, antequam ad quadraturas Curvarum, vel potius figurarum curvilinearum, || veniat, præmittit brevem Isagogen. Quæ ** ut meliùs intelligatur, sciendum est, cum magnitudo aliqua continuè crescit, veluti linea, exempli gratiâ, crescit fluxus puncti quod eam describit, ++ incrementa illa momentanea appellari differentias; nempe inter magnitudinem quæ antea erat, & quæ per mutationem momentaneam est producta; atque hinc natum esse calculum differentialem, eique reciprocum summatorium; cujus elementa ab inventore D. Godefrido Guiljelmo Leibnitio in his Actis sunt tradita; variiq; usus tum ab ipso, tum à D. D. Fratribus Bernoulliis, tum & D. Marchione Hospitalio (cujus nuper extincti immaturam mortem omnes magnopere dolere debent, qui profundioris doctrinæ profectum amanti) sunt ostensi. Pro differentiis igitur Leibnitianis D. Newtonus †† adhibet, semperque adhibuit, fluxiones; quæ sunt quàm proximè ut fluentium argumenta, æqualibus temporis particulis quàm minimis genita; iisque, tum in suis Principiis Naturæ Mathematicis, tum in aliis postea editis, eleganter est usus; quemadmodum & Honoratus Fabrius in suâ Synopsi Geometricâ, motuum progressus Cavalierianæ methodo substituit.

Subinde editores vice symbolorum Newtoni describunt symbola Leibnitii, & postea librum Newtoni sic breviter attingunt.

Cum regressus à differentiis ad quantitates, vel à quantitatibus ad summas, vel denique à fluxionibus ad fluentes non semper algebraicè fieri possit; ideo quærendum est, tum quibus casibus quadratura algebraicè succedat, tum quomodo, algebraico successu deficiente, aliquid subsidiarium adhiberi queat. In utroque enim à D. Newtono est utilissimè laboratum, tum aliàs, tum in hoc tractatu de quadraturis; ubi

** Ut Isagoge meliùs intelligatur, Leibnitius describit calculum suum differentialem, & omittit calculum Newtonianum, quem solum describere debuisset. Hoc fecit, non ut calculus Newtonianus, in Isagoge traditus meliùs intelligatur, sed ut rejiciatur.

†† Incrementa illa momentanea Newtonus momenta, Leibnitius postea differentias vocavit. Et inde natum est nomen calculi differentialis.

‡‡ Sensus verborum est, quod Newtonus fluxiones differentii Leibnitianis substituit, quemadmodum Honoratus Fabrius motuum progressus Cavalierianæ methodo substituerat; id est, quod Leibnitius autor primus fuit hujus methodi, & Newtonus eandem à Leibnitio habuit, substituendo fluxiones pro differentiis.

*series adhibet infinitas; quæ, eo casu quo abruptuntur, seu finiuntur, quæstium algebraicæ exhibent. De * quo etiam dictum est nuper in recensione tractatus D. Cheynæi, Medici Scoti Londini degentis. Conferri etiam potest tractatus D. Craigii Scoti de Quadraturis, & ejusdem theorema ad quadraturas pertinet, nuper in his Actis exhibitum; quæ faciunt etiam, ut ipsis theorematibus Newtonianis recensendis supersedeamus, quia paucis exponi non possunt: quemadmodum nec ejusdem theoremata quædam reductionis ad quadraturas faciliores.*

His permotus D. Joannes Keill, in epistolâ in Philosophicis Transactionibus A. C. 1708, mensibus Septemb. & Octob. impressâ, scripsit in contrarium, quod Fluxionum Arithmetica, sine omni dubio, primus invenit Dominus Newtonus, ut cuilibet ejus epistolas, à Wallisio editas, legenti faciliè constabit. Eadem tamen arithmetica postea, mutatis nomine & notationis modo, à Domino Leibnitio in Actis Eruditorum edita est.

N°LXXX. *Epistola D. Leibnitii ad D. Hans Sloane Regiæ Societatis Secretarium, 4 Martii S. N. 1711 data.*

Gratias ago, quod novissimum volumen præclari operis Transactionum Philosophicarum ad me misisti; quamvis nunc demum mihi, Berolinum excurrenti, redditum sit. Itaque excusabis, quod pro munere superioris anni nunc demum gratiæ, dudum debitiæ, redduntur.

Vellem inspectio operis me non cogeret, nunc secundâ vice, ad vos querelam deferre. Olim Nicolaus Fatius Duillierius me pupugerat, in publico scripto, tanquam alienum inventum mihi attribuissim. Ego eum in Actis Eruditorum Lipsiensibus meliora docui; & vos ipsi, ut ex literis à Secretario Societatis vestræ inclytæ (id est, quantum memini, à teipso) scriptis didici, hoc improbâstis. Improbavit Newtonus ipse vir excellentissimus, quantum intellexi, præposterum quorundam hæc in re erga vestram gentem & se studium. Et tandem D. Keillius in hoc ipso volumine, mense Sept. Octob. 1708, pag. 185; renovare ineptissimam accusationem visus est; cum scripsit, *Fluxionum Arithmetica à Newtono inventam, mutato nomine & notationis modo, à me editam fuisse.* Quæ qui legit, & credit, non potest non suspicari, alterius inventum à me, larvatum subdititiis nominibus characteribusque, fuisse protrusum. Id quidem quàm falsum sit, nemo melius ipso D. Newtono novit. Certè ego nec nomen calculi fluxionum fando audi; nec characteres, quos adhibuit D. Newtonus, his oculis vidi, antequam in Wallisianis operibus prodire. Rem etiam me habuisse, multis antè annis quàm edidi, ipsæ literæ apud Wallisium editæ demonstrant. Quomodo ergo aliena mutata edidi, quæ ignorabam.

Etsi autem D. Keillium, à quo magis præcipiti judicio quàm malo animo peccatum puto, pro calumniatore non habeam; non possum tamen non ipsam accusationem in me injuriam pro calumniâ habere. Et quia verendum est,

* Sensus est, quod quæ Newtonus habet in hoc tractatu de quadraturis, & speciatim de quadraturis illis ubi series abruptuntur, vel finiuntur, à Cheynæo & Craigio prius dicta sunt, & in his Actis nuper exhibitæ; quæ, quia multa sunt, faciunt, ut à Newtonianis recensendis editores Actorum

est, ne sæpe vel ab improbis, vel ab imprudentibus repetatur; cogor remedium ab inclytâ vestrâ Societate Regiâ petere. Nempe æquum esse vos ipsi, credo, judicabitis, ut D. Keillius testetur publicè, non fuisse sibi animum imputandi mihi, quod verba insinuare videntur, quasi ab alio hoc quicquid est inventi didicerim, & mihi attribuerim. Ita ille & mihi læso satisfaciet, & calumniandi animum à se alienum esse ostendet; & aliis, aliâs similia aliquando jactaturis, frænum injicietur. Quod superest vale & fave.

Dabam Berolini 4 Martii 1711.

Epistola D. Johannis Keill, A. M. ex Æde Christi Oxon. R. S. Socii, & N°LXXXI. jam Astronomiæ Professoris Saviliani, ad D. Hans Sloane, M. D. Regiæ Societatis Secretarium, cum D. Leibnitio communicanda.

Cum D. Leibnitii epistolam mecum Vir Cl. communicare dignatus sis; ea etiam, quæ mihi visum fuerit rescribere, ne graveris accipere. Sentio virum egregium acerrimè de me queri, quasi ei injuriam fecerim, & rerum à se inventarum gloriam aliò transfulerim. Fateor querelam hanc ideo mihi molestam esse; quod nolim, ea sit de me hominum opinio, quasi ego calumniandi studio, cuiquam in rebus mathematicis versanti, nedum viro in iisdem versatissimo, obtrectarem; certè nihil ab ingenio meo magis alienum est, quàm alterius laboribus quicquam detrahere.

Agnosco me dixisse fluxionum Arithmetica à D. Newtono inventam fuisse, quæ mutato nomine & notationis modo, à Leibnitio edita fuit. Sed nollem hæc verba ita accipi; quasi aut nomen, quod methodo suæ imposuit Newtonus, aut notationis formam, quam adhibuit, D. Leibnitio innotuisse contenderem: sed hoc solum innuebam, D. Newtonum fuisse primum inventorem Arithmetice fluxionum, seu calculi differentialis; eum autem in duabus ad Oldenburgum scriptis epistolis, & ab illo ad Leibnitium transmissis, indicia dedisse, perspicacissimi ingenii viro satis obvia, unde Leibnitius principia istius calculi hausit, vel saltem haurire potuit: at cum loquendi & notandi formulas, quibus usus est Newtonus, ratiocinando assequi nequiret vir illustris, suas imposuit.

Hæc ut scriberem, impulerunt Actorum Lipsiensium Editores; qui in eâ quam exhibent, operis Newtoniani de fluxionibus seu quadraturis enarratione, disertè affirmant D. Leibnitium fuisse istius methodi inventorem; & Newtonum aiunt pro differentiis Leibnitianis fluxiones adhibere, semperque adhibuisse. Id quidem in iisdem scriptoribus observatu dignum; quod loquendi & notandi formam, à Newtono adhibitam, in Leibnitianam passim in eadem enarratione transferunt; de differentiis scilicet, & summis, & calculo summatorio loquuntur, de quibus est nullus apud Newtonum sermo: quasi inventa Newtoni Leibnitianis posteriora fuerint, & à calculo Leibnitii, in Actis Lipsiensibus anno 1684 descripto, ortum derivarint. Cum reverâ Newtonus, ut ex sequentibus patebit, fluxionum methodum invenierit, octodecim saltem annos antequam Leibnitius quicquam de calculo dif-

ferentialem superseant. Et eodem sensu D. Leibnitius ad Secretarium Societatis Regiæ nuper scripsit, suum cuique hic redditum esse, quasi secundam Newtoni ad Oldenburgum epistolam, ad se missam, & supra impressam nunquam legisset. Vide pag. 544—547.

ferentiali edidisset, tractatumque de eâ re conscripserit; cujus cum specimina quædam Leibnitio ostensa sint, rationi non incongruum est, ea aditum illi ad calculum differentialem aperuisse.

Unde si quid de Leibnitio liberius dixisse videar, id eo animo feci, non ut ei quicquam eriperem; sed ut quod Newtoni esse arbitrabar, auctori suo vindicarem.

Maxima equidem esse Leibnitii in rempublicam literariam merita, lubens agnosco; nec eum in reconditiore mathesi scientissimum esse, diffitebitur, qui ejus in Actis Lipsiensibus scripta perlegerit. Cum autem tantas tamque indubitatas opes de proprio possideat; certè non video, cur spoliis ab aliis detractis onerandus sit. Quare cum intellexerim, populares suos ita illi favere, ut eum laudibus non suis accumularent; haud præposterum in gentem nostram studium esse duxi, si Newtono, quod suum est, tueri & conservare anniterer: nam si Lipsiensibus fas fuerit, alieba Leibnitio affingere, Britannis saltem ea, quæ à Newtono erepta sunt, sine crimine calumniæ reposcere licebit. Itaque cum ad Regiam Societatem appellet vir illustris, meque publicè testari velit, calumniandi animum à me alienum esse; ut calumniandi crimen à me amoveam, mihi ostendendum incumbit, D. Newtonum verum & primum fuisse Arithmeticæ fluxionum, seu calculi differentialis, inventorem; deinde ipsum adeo clara & obvia methodi suæ indicia Leibnitio dedisse, ut inde ipsi facile fuerit, in eandem methodum incidere.

Sciendum verò primum est, celeberrimos tunc temporis geometras, Dominos Franciscum Slusium, Isaacum Barrovium, & Jacobum Gregorium, methodum habuisse, quâ Curvarum tangentes ducebant, quæ à fluxionum methodo non multum abludebat; & iisdem principiis innixa fuit. Nam si pro literâ o , quæ in Jacobi Gregorii Parte Matheseos Universalis quantitatem infinitè parvam repræsentat; aut pro literis a vel e , quas ad eandem designandam adhibet Barrovius; ponamus x vel y Newtoni, vel dx seu dy Leibnitii; in formulas fluxionum, vel calculi differentialis, incidemus: & regressus, quo à datâ tangentium proprietate ad naturam Curvæ perveniebant, quem methodum tangentium inversam nominabant, eadem plane res erat, ac methodus quâ à fluxionibus ad fluentes revertitur: interim suam methodum non ultra fluxiones primas extendebant; neque eandem ad quantitates surdis aut fractionibus involutas accommodare potuerunt. At prius quàm quicquam de hoc argumento à summis hisce viris publico datum est, D. Newtonus methodum excogitavit, priori quidem non dissimilem, sed multo latius patentem; quæ non substituit ad æquationes eas, in quibus una vel utraque quantitas indefinita radicalibus est involuta; sed, absque ullo æquationum apparatu, tangentem confestim ducere monstrabat; quæstiones de maximis & minimis eodem artificio tractabat; & speculationes de quadraturis facilius explicuit. Hæc constant ex epistolâ Newtoni, ad D. Collinium datâ, Decembris die 10, anno 1672, & inter Collinii chartas reperiâ.

Hæc epistola habetur impressa N° XXVI.

Ex

Ex hac epistolâ clarè constat, D. Newtonum methodum fluxionum habuisse ante annum 1670, eodem nempe quo Barrovii Lectiones editæ sunt.

Anno 1669 misit Newtonus ad D. Collinium tractatum de Analyti per N° LXXXII. *Æquationes Infinitas*; quem etiam, inter schedas Collinii repertum, D. Jones nuper edidit. Sub hujus sine habetur demonstratio regulæ pro Quadraturis Curvarum, nata ex proportionem augmentorum nascentium abscissæ &

ordinatæ, cum abscissâ sit z , & ordinata $x^{\frac{p}{q}}$; quæ quidem demonstratio commune fundamentum est tam doctrinæ fluxionum, quàm calculi differentialis. Ex eo autem tractatu non pauca, amicis suis communicanda, deprompsit Collinius: unde certum est, D. Newtono, ante illud tempus, fluxionum Arithmeticam innotuisse. Præterea constat ex posteriore Newtoni ad Oldenburgum epistolâ, eum, suadentibus amicis, circa annum 1671 tractatum de hisce rebus conscripisse; quem, unâ cum theoriâ lucis & colorum, in publicum dare statuerat: scribitque Oldenburgum, series infinitas non magnam ibi obtinuisse partem; seque alia haud pauca congestisse, inter quæ erat methodus ducendi tangentes, quam solertissimus Slusius, ante annos duos trese, cum Oldenburgum communicaverat; sed quæ generalior facta, non ad æquationes, quæ surdis aut fractionibus involutæ sunt, hærebat; & eodem fundamento usum, ad theorematum generalia, quadraturas Curvarum spectantia, pervenisse se ait Newtonus. Horum unum exempli loco in ipsâ epistolâ ponit; seriem exhibens, cujus termini dant quadraturam Curvæ, cum abscissâ est z & ordinatam applicata $dz^{\frac{p}{q}} \times e + \sqrt{z}$. Quæ series abruptitur, & terminis finitis Curvæ quadraturam comprehendit, quodcumque illa finitâ æquatione exprimi potest. Hoc dicit esse primum theorematum generaliorum; unde sequitur, eum alia, ad casus difficiliore & magis intricatos accommodata, habuisse: est autem theorema illud Propositio v. in tractatu de quadraturis. Eodem etiam spectat ejusdem Prop. vi. sed ad casus magis implicatos se extendit. Propositiones tertia & quarta sunt lemmata Theor. hisce demonstrandis præmissa. Secunda autem in quadraturis Propositio extat in tractatu de Analyti per *Æquationes Infinitas*, & prima propositio est ea ipsa, quam in dictâ epistolâ fundamentum operationum vocat, & transpositis literis celari tunc voluit. Scribit etiam Newtonus, se dudum theorematum quedam, quæ comparationi Curvarum cum sectionibus conicis infervant, in catalogum retulisse; & ordinatas Curvarum, quæ ad eam normam comparari possunt, in eadem epistolâ describit; quæ profecto eadem planè sunt cum iis, quas tabula secunda ad scholium Propositionis x. in tractatu de quadraturis, exhibet; unde satis liquet tabulam illam & Propositiones VII, VIII, IX & X, quæ sunt in tractatu de quadraturis (à quibus tabula pendet) Newtonum dudum invenisse ante annum 1676, quo scripta est epistola illa posterior. Cum verò, in primâ ad Oldenburgum epistolâ, dicit se ab ejusmodi studiis per quinquennium abstinuisset; hinc satis clarè colligi potest, propositiones in tractatu de quadraturis à D. Newtono inventas fuisse, quinquennio saltem antequam epistolæ illæ ad Oldenburgum scrip-

ta.

tæ essent; totamque illam de fluxionibus doctrinam, ante illud tempus ulterius à Newtono provectam esse, quàm ad hunc usque diem à quoquam alio factum est sub nomine calculi differentialis. Certè neminem novi, qui, in hac provinciâ peragranda, æquis passibus cum Newtono progressus sit: & pauci sunt, iique insignes geometræ, qui prospicere queant, quousque ille in eâdem provinciâ processerit. Præterea, in posteriore illâ ad Oldenburgum epistolâ, modum describit, quo in seriem inciderit, cujus termini fluxiones, seu differentias, quantitatum in infinitum exhibent; quæ postquam inventa esset, dicit, pestem ingruentem ipsum coegisse hæc studia deferere, & alia cogitare. At pestis illa contigit annis 1665 & 1666; unde patet, etiam ante illud tempus fluxionum calculum D. Newtono innotuisse; hoc est duodecim saltem annos antequam calculum suum Oldenburgo communicavit Leibnitius; & novemdecim annos antequam Vir Illustris eandem in Actis Lipsiensibus edidit: & certè ante vitas hæce duas Newtoni epistolæ, Leibnitium calculum suum differentialem habuisse, nulla apparent vestigia. His omnibus ritè perpensis, certissimè cuivis constabit, D. Newtonum pro vero inventore Arithmeticæ fluxionum habendum esse.

NºLXXXIII. Restat jam ut inquiramus, quænam fuere indicia Leibnitio à Newtono derivata, unde ei facile foret calculum differentialem elicere. Et primo, ut dixi, nullibi ostendit Leibnitius sibi notum fuisse calculum differentialem, ante visas has duas Newtoni epistolæ; imo ante illud tempus longiore usus est circuitu, cum res facilius multo & succinctius ex calculo fluenter differentiali. Hujus rei testis sit epistola, ad Oldenburgum data 1/2 Novembris 1676, quæ in Operum Wallisianorum tomo tertio etiam extat, in quâ modum tradit exprimendi rationem subtangentis ad ordinatam, in terminis quos non ingreditur ordinata; ubi si loco y & dy ipsarum valores vinculo inclusos posuisset, statim scopum attigisset.

In primâ epistolâ, quæ per Oldenburgum ad Leibnitium transmissa est, docuit Newtonus methodum, quâ quantitates in series infinitas reducendæ sint, i. e. quâ quantitatum fluentium incrementa exhiberi possunt. In ipso enim initio seriem ostendit, cujus termini hæc incrementa repræsentant. Sed illa D. Leibnitium prorsus latebat, ante visam Newtoni epistolam, quâ exponitur.

Sit o incrementum momentaneum quantitatis fluentis x , & $\frac{m}{n}$ index dignitatis ejusdem, & si pro x scribatur $x+o$, $x+2o$, $x+3o$, $x+4o$, &c.

& quantitates $\overline{x+o}^{\frac{m}{n}}$, $\overline{x+2o}^{\frac{m}{n}}$, $\overline{x+3o}^{\frac{m}{n}}$, $\overline{x+4o}^{\frac{m}{n}}$, &c. in series infinitas expandantur; habebimus totidem series, quarum prima hæc est, quæ sequitur: $x^{\frac{m}{n}} + \frac{m}{n} o x^{\frac{m-n}{n}} + \frac{m^2 - mn}{2n^2} o o x^{\frac{m-2n}{n}} + \frac{m^3 - 3m^2n + 2mn^2}{6n^3} o o o x^{\frac{m-3n}{n}}$ &c. In om-

nibus seriebus primus terminus erit ipsa quantitas fluens $x^{\frac{m}{n}}$; & si prior quælibet series à posteriore auferatur, habebimus harum serierum differentias primas; in quibus omnibus primus terminus est seriei primæ terminus primus,

primus, quem ingreditur quantitas o ; scilicet $\frac{m}{n} o x^{\frac{m-n}{n}}$: & evanescente o , fit ille terminus differentis hîc primis æqualis; vel quod idem est, erit quantitas $\frac{m}{n} o x^{\frac{m-n}{n}}$ fluentis incrementum primum.

Præterea si differentia quælibet prior à posteriore auferatur, deveniemus ad differentias secundas; quarum omnium terminus primus per 2 divisus, idem est cum termino secundo seriei primæ, quem ingreditur quantitas o ; & evanescente o , fiunt differentiæ illæ per binarium divisæ singulæ æquales termino illi primo seriei, qui est $\frac{m^2 - mn}{2n^2} o o x^{\frac{m-2n}{n}}$. Et eodem modo invenie-

mus suprâ descriptæ seriei terminum $\frac{m^3 - 3m^2n + 2mn^2}{6n^3} o o o x^{\frac{m-3n}{n}}$, æqualem esse singulis differentis tertiis, per sex divisus. Et quilibet terminus ejusdem seriei ad differentias respectivas semper habebit datam rationem; scilicet terminus primus, quem ingreditur o , æqualis est differentis primis; secundus est differentiarum secundarum pars media; tertius, pars sexta differentiarum tertiarum, &c. Hæc series, quarum termini differentias omnes in infinitum repræsentant, invenit Newtonus, uti dixi, ante annum 1665; sed illæ ante visam Newtoni epistolam, in quâ exponitur, D. Leibnitium * latebant: nam ante illud tempus agnoscit Leibnitius, semper ipsi necesse fuisse transmutare quantitatem irrationalem in fractionem rationalem, & deinde, dividendo Mercatoris methodo, fractionem in seriem reducere. Exinde etiam patet seriem hanc, differentias continentem, non habuisse D. Leibnitium, quòd postquam ipsi per Oldenburgum ostensa est, * rogat, ut D. Newtonus ipsius originem sibi pandat.

Sit jam quantitas quælibet, ex constanti & indeterminatis utunque composita, & vinculo inclusa; scilicet $\overline{a+bx}^{\frac{m}{n}}$; cujus differentia habenda est. Constat, per regulam priùs traditam, quantitatis $a+bx$ differentiam esse $cbx^{c-1}o$, posito quòd o sit incrementum momentaneum fluentis x . Quare si

pro $a+bx$ scribatur z , & pro $cbx^{c-1}o$ scribatur ω , erit $\overline{a+bx+c bx^{c-1}o}^{\frac{m}{n}} = \overline{z+\omega}^{\frac{m}{n}}$; quæ si per regulam Newtoni in seriem expandatur, fit $z^{\frac{m}{n}} + \frac{m}{n} \omega z^{\frac{m-n}{n}} + \&c.$ cujus seriei terminus $\frac{m}{n} \omega z^{\frac{m-n}{n}}$ est differentia prima quantitatis $z^{\frac{m}{n}}$, seu $\overline{a+bx}^{\frac{m}{n}}$. Unde si loco z & ω restituantur ipsorum valores,

$a+bx$ & $cbx^{c-1}o$, habebimus differentiam quantitatis $\overline{a+bx}^{\frac{m}{n}} = \frac{m}{n} cbx^{c-1}o \times \overline{a+bx}^{\frac{m-n}{n}}$: vel si more Leibnitiano pro o ponatur dx , erit quantitatis $\overline{a+bx}^{\frac{m}{n}}$ differentia $\frac{m}{n} cbx^{c-1}dx \times \overline{a+bx}^{\frac{m-n}{n}}$; ubi videmus quantitatem dif-

* Vide epistolam Leibnitii ad Oldenburgum 27 Augusti 1676, pag. 537.

ferentialem, $\frac{m}{n} cbx^{-1} dx$, extra vinculum semper manere. Atque hinc facile fuit D. Leibnitio, ope regulæ Newtonianæ, differentias quantitatum omnium exhibere, utcumque quantitates fluentes surdis aut fractionibus sint implicatæ: id quod ante epistolicum illud, per Oldenburgum, cum Newtono commercium ipsi minimè notum fuit.

Quamvis hæc per se satis manifesta sunt calculi differentialis indicia; in secundâ tamen epistolâ, quæ per Oldenburgum ad Leibnitium missa est, alias adhuc clariores describit Newtonus methodi suæ notas. Dicit enim, se habuisse methodum ducendi tangentes, quam solertissimus Slusius ante annos duos trefve Oldenburgo impertitus est, ita ut habito suo fundamento nemo posset tangentes aliter ducere, nisi de industriâ à recto tramite erraret. Quinetiam ibi quoque ostendit "Methodum hanc non hære ad æquationes, quibus una vel utraque quantitas indefinita radicalibus involuta est; sed absque ullâ æquationum reductione, quæ opus plerumque redderet immensum, tangentem confestim duci; & eodem modo in quæstionibus de Maximis & Minimis, aliisque quibusdam, rem sic se habere. "Fundamentum harum operationum dicit esse satis obvium, quod tamen transpositis literis in illâ epistolâ celare voluit. Hoc etiam adjicit; hoc fundamentum speculationes de quadraturis Curvarum simpliciores se reddidisse; & ad theorematâ quædam generalia se pervenisse scribit."

Cum verò methodus Slusiana tunc temporis Leibnitium minimè latere potuit, utpote in Actis Philosophicis Londini publicata: cumque Newtonus dicit, eandem & sibi innotuisse, ex fundamento, quo habito non hærebat ad æquationes radicalibus utcumque involutas; in quâ quidem tota rei difficultas posita est: cumque in priore epistolâ seriem descripsit, cujus ope differentia haberi possunt, ubi fluentes surdis aut fractionibus utcumque implicatæ sunt: cum denique idem fundamentum ad quadraturas Curvarum se applicuisse dicit: minimè dubitandum est, hæc omnia facem Leibnitio prætulisse, quo facilius methodum Newtoni perspiceret.

N^o LXXXIV. Quod si hæc non suffecisse videantur indicia; etiam ulterius processit Newtonus, & exempla methodi suæ dedit, & regulam ostendit, quâ ex datis quarundam Curvarum ordinatis, earundem areæ exhibentur in terminis finitis, cum hoc fieri potest; hoc est, in stylo Leibnitiano, ipsi exempla tradidit, quibus à differentiis ad summas pervenitur. Et à simplicioribus orsus, * proponit primo parabolam, cujus abscissa est z , & ordinatim applicata $\sqrt{az} = a^{\frac{1}{2}} z^{\frac{1}{2}}$; & Curvæ area erit $\frac{2}{3} a^{\frac{1}{2}} z^{\frac{3}{2}}$; hoc est, quando differentia areæ est $dz \times \sqrt{az}$, seu $a^{\frac{1}{2}} z^{\frac{1}{2}} \times dz$, ostendit fore aream $\frac{2}{3} a^{\frac{1}{2}} z^{\frac{3}{2}}$; unde vicissim concluditur, si quantitas differentianda sit $a^{\frac{1}{2}} z^{\frac{1}{2}}$, fore ejus differentiam $\frac{1}{2} a^{\frac{1}{2}} z^{-\frac{1}{2}} dz$ seu $\frac{1}{2} dz \sqrt{az}$. Exemplum ejus secundum est Curva, cujus abscissa est z , & ordinatim applicata $\frac{a^{\frac{1}{2}} z}{c - z^2}$: ubi ostendit Newtonus Curvæ aream fore $\frac{a^{\frac{1}{2}}}{2c^2 - 2z^2}$: hoc est, si differentia areæ sit $\frac{a^{\frac{1}{2}} dz}{c^2 - z^2}$, ostendit aream

* Vide pag. 545.

fore $\frac{a^{\frac{1}{2}}}{2c^2 - 2z^2}$. Unde vicissim si quantitas differentianda sit $\frac{a^{\frac{1}{2}}}{2c^2 - 2z^2}$, concludi potest differentiam fore $\frac{a^{\frac{1}{2}} \times dz}{c^2 - z^2}$. Vel si ejusdem Curvæ ordinata sic enuncietur, $\frac{a^{\frac{1}{2}}}{z^2 \times c^2 z^{-1} - 1}$; erit area $= \frac{a^{\frac{1}{2}} z^2}{2c^2 - 2z^2}$. Quare & vicissim, si quantitas differentianda sit $\frac{a^{\frac{1}{2}} z^2}{2c^2 - 2z^2}$, erit differentia $\frac{a^{\frac{1}{2}} dz}{z^2 \times c^2 z^{-1} - 1}$.

Hinc ad exempla quædam difficiliora progreditur Newtonus; in iisque ostendit, quomodo ab ordinatis, hoc est à differentiis, ad summas perveniendum sit: ex quibus patebit, Curvam omnem quadrabilem fore, cujus ordinata, in differentiam abscissæ ducta, sit quantitatis alicujus differentia; & hinc innumera Curvarum genera assignari possunt, etiam geometricè quadrabilia.

His indiciis atque his adjutum exemplis, ingenium vulgare methodum Newtonianum penitus discerneret; ita ut ne suspicari fas sit, eam acerrimum Leibnitii acumen posse latuisse; quem quidem usum fuisse his ipsis clavibus, ad hæc sua quæ feruntur inventa, aditum, etiam ex ipsius ore satis elucescit. Nam in epistolâ ad Oldenburgum datâ, post explicatum calculum differentialem, exemplum addit, quod coincidere agnoscit cum regulâ Slusianâ; & postea addit. * "Sed methodus ipsa priore nostrâ longè est amplior: non tantum enim exhiberi potest; cum plures sint li-
" teræ indeterminatæ quàm x & y ; quod sæpe fit maximo cum fructu: sed
" & tunc utilis est, cum interveniunt irrationales; quippe quæ eam nullo
" morantur modo, neque ullo modo necesse est irrationales tolli; quod in
" regulâ Slusii necesse est, & calculi difficultatem in immensum auget." Hæc omnia à Newtono prius, in secundâ ejus epistolâ, dicta sunt. Inde exempla proponit, quorum quidem quod primum est, nescio quo fato, idem prorsus est ac id, quod, in eâ epistolâ quam Leibnitio transmisserat Oldenburgus, etiam primum protulerit Newtonus.

Mox addit vir illustrissimus, * "Arbitror, quæ celare voluit Newtonus de tangentibus ducendis, ab his non abludere. Quod addit, ex hoc fundamento quadraturas quoque reddi faciliores, me in hac sententiâ confirmat. Nimirum semper figuræ illæ sunt quadrabiles, quæ sunt ad æquationem differentialem. Æquationem differentialem voco talem, quâ valor ipsius dx exprimitur, quæque ex aliâ derivata est, quâ valor ipsius x exprimebatur." Et paulo post, suam de hac re sententiam plenius aperit; dicitque hanc unicam regulam pro infinitis figuris quadrandis infervere, diversæ planè naturæ ab iis quæ hactenus quadrari solebant. Quis est jam, qui hæc perpendet, & non videbit, indicia & exempla Newtoni satis à Leibnitio perspecta fuisse; saltem quoad differentias primas? Nam quoad differentias secundas, Leibnitium methodum Newtonianam tardiùs intellexisse videtur, quod brevi forsitan clarius monstrabo.

Interim facile illustri viro assentior, & credo eum nec nomen calculi fluxionum fando audivisse; nec characteres, quos adhibuit Newtonus, oculis

* Vide pag. 560, 561.

vidisse, ante quàm in Wallisianis operibus prodire. Observo enim, ipsum Newtonum sapius mutasse nomen & notationem calculi. In tractatu de Analyti Aëuationum per Series Infinitas, incrementum abscissæ per litteram *o* designat: et in Principiis Philosophiæ fluentem quantitatem genitam vocat, ejusque incrementum momentum appellat: illam litteris majoribus, *A* vel *B*, hoc minusculis, *a* & *b*, designat.

Id etiam ultrà agnosco; inter cætera quæ de re mathematicâ præclare meritis est Leibnitius, hoc itidem illi deberi; quòd primus fuerit, qui calculum hunc typis edidit, & in publicum produxit: itaque eo saltem nomine magnam apud mathefos amantes inibit gratiam, quòd inventum ita nobile, & in multiplices usus deducendum, diutius eos noluerit latere.

Habes, Vir Cl. quæ de hoc argumento scribenda duxi; unde faciliè credo percipies, hoc quaecunque fuerit meum in gentem nostram studium, ita parum præposterum fuisse, ut nihil omnino nisi quod Newtoni erat, Leibnitio detraxerim; nec dubito, quin æqui rerum æstimatores uno ore fateantur me, uti nullo calumniandi animo, ita nec præcipiti judicio, ea dixisse, quæ tibi tot argumentis luce meridianâ clariùs comprobavi.

Leeta est hæc epistola coram Regiâ Societate, in conventu die 24 Maii 1711 habito; & ut exemplar ejus D. Leibnitio mitteretur, D. Sloane Secretario suo mandatum est.

Nº LXXXV.

Epistola D. Leibnitii ad D. Hans Sloane M. D. & R. S. Sec.

Quæ D. Johannes Keillius nuper ad te scripsit, candorem meum apertius quàm antè oppugnant: quem ut ego hâc ætate, post tot documenta vitæ, apologiâ defendam; & cum homine docto, sed novo, & parum perito rerum anteaclarum cognitore, nec * mandatum habente ab eo cujus interest, tanquam pro tribunali litigem; nemo prudens æquusque probabit.

Quæ ille de meo rem cognoscendi modo suspicatur, haud satis exercitatus artis inveniendi arbiter, ipsius quidem docendi causâ non est cur resellam: sed nõrunt † amici, quàm longè alio, & ad alia proficuo, itinere processerim. Frustra ad exemplum Actorum Lipsiensium provocat, ut sua dicta excuset; in illis enim circa hanc rem quicquam cuiquam detractum non reperio, sed potius passim ‡ suum cuique tributum. Ego quoque & amici

* Quasi methodum Montoni, & series Brounkeri, Wallisi & Gregorii, aliorumque inventa non liceat propriis authoribus, nisi autoritate ab his acceptâ, asserere.

† Si amici illi sunt Germani, invenit is hanc methodum post reditum suum in Germaniam.

‡ Scripserit Keillius in hæc verba. *Hæc ut scriberem, impulerunt Actorum Lipsiensium Editores; qui, in eâ quam exhibent operis Newtoniani de fluxionibus seu quadraturis enarratione, disertè affirmant, D. Leibnitium fuisse ipsius methodi inventorem, & Newtonum aiunt pro differentiis Leibnitianis fluxiones adhibere semperque adhibuisse.* Leibnitius editores hîc palam defendit contra Keillium, quasi suum cuique reddidissent.

§ In epistolâ Aug. 27, 1676, properavit se coinventorem methodi serierum proponere. In epistolâ Junii 21, 1677, properavit methodum ut suam describere, de quâ Newtonus tractatum ante annos quinque scripserat. In schedis tribus anno 1689 impressis, properavit propositiones principales Principiorum Philosophiæ ad calculum suum revocatas in lucem edere, ut in inventoris jura veniret.

¶ Probandum est.

aliquoties

aliquoties ostendimus, libenter à nobis credi, illustrem Fluxionum autorem per se ad similia nostris fundamenta pervenisse. Neque eo minùs ego in inventoris jura venio; quæ etiam Hugenus, judex intelligentissimus incorruptissimisque, publicè agnovit: in quibus tamen mihi vendicandis § non properavi; sed inventum || plusquam nonum in annum pressi, ut nemo me præcurrisse queri possit.

Itaque vestræ æquitati committo, annon coercendæ sint vanæ & injustæ vociferationes; quas ** ipsi Newtono, viro insigni, & gestorum optime conficio, improbari arbitror: ejusque sententiæ suæ libenter daturum indicia mihi persuadeo.

VALE. Dabam Hannoveræ 29 Decemb. 1711.

Cùm D. Leibnitius à D. Keill, ut homine novo, ad Societatem Regiam pro-NºLXXXVI vocaret; Societas jussit monumenta antiquiora consuli; & fociis aliquot, qui his examinandis aptiores viderentur, in mandatis dedit, ut in hanc rem inquirerent; & quæ in scriptis antiquis invenirent, ad se referrent, unâ cum eorum sententiâ. Et arbitrorum confessus collectionem ex epistolis & aliis MSS. suprâ impressam ad Societatem retulerunt, unâ cum eorum sententiâ sequente.

We have consulted the letters and letter-books in the custody of the Royal Society, and these found among the papers of Mr. John Collins, dated between the years 1669 and 1677 inclusive; and shewed them to such as knew and avouched the hands of Mr. Barrow, Mr. Collins, Mr. Oldenburg, and Mr. Leibnitz; and compared those of Mr. Gregory with one another, and with copies of some of them taken in the hand of Mr. Collins; and have extracted from them what relates to the matter referred to us; all which extracts herewith delivered to you, we believe to be genuine and authentick. And by these letters and papers we find,

1. *That Mr. Leibnitz was in London in the beginning of the year 1673; and went thence in or about March to Paris; where he kept a correspondence with Mr. Collins, by means of Mr. Oldenburg, till about September 1676, and then returned by London and Amsterdam to Hanover: and that Mr. Collins*

** Newtonus & Leibnitius nec sunt idonei judices nec testes. Ex monumentis antiquis judicium ferendum est.

LITERAS & literarum apographa, tam quæ in archivis Regiæ Societatis, quàm quæ inter chartas D. Joannis Collinii afferantur, & inter annos 1669 & 1677 datæ sunt, inspeximus; & ex his, quæ D. Barrovii, D. Collinii, D. Oldenburgi & D. Leibnitii nomen ferebant, ex fide aliquorum qui eorum autographa probè noverant, ipsorum esse certo didicimus. Literas autem, quæ Gregorius præ se ferebant auctorem, ipsius esse cognovimus fide Collinii; qui nonnullas earum, Gregorius assignatas, manu suâ exscripserat. Ex his omnibus excerptimus, quæcunque ad rem nobis commissam pertinere videbantur; atque illa excerpta, quæ unâ cum ipsis literis jam vobis traduntur, fideliter & accuratè facta esse comperimus. Ex his autem literis chartisque nobis constat;

1. D. Leibnitium, anno ineunte 1673, Londini fuisse; unde mense Martio, vel circiter, Parisios adiit; ubi literarum commercium habuit cum D. Collinio, intercedente Oldenburg, usque in mensem Septembrem 1676: deinde per Londinum & Amstelodamum Hannoveram reversum esse: D. autem Collinium mathefos peritis ea, quæ à D. Newtono & Gregorio acceperat, libentissimè communicasse.

was very free in communicating, to able mathematicians, what he had received from Mr. Newton and Mr. Gregory.

II. That when Mr. Leibnitz was the first time in London, he contended for the invention of another Differential Method properly so called; and notwithstanding that he was shewn by Dr. Pell, that it was Mouton's method, persisted in maintaining it to be his own invention, by reason that he had found it by himself, without knowing what Mouton had done before, and had much improved it. And we find no mention of his having any other Differential Method than Mouton's, before his letter of the 21st of June 1677, which was a year after a copy of Mr. Newton's letter, of the 10th of December 1672, had been sent to Paris to be communicated to him; and above four years after Mr. Collins began to communicate that letter to his correspondents; in which letter, the method of Fluxions was sufficiently described to any intelligent person.

III. That by Mr. Newton's letter of the 13th of June 1676 it appears, that he had the method of Fluxions above five years before the writing of that letter. And by his Analysis per Aequationes numero Terminorum Infinitas, communicated by Dr. Barrow to Mr. Collins in July 1669, we find that he had invented the method before that time.

IV. That the Differential Method is one and the same with the Method of Fluxions, excepting the name and mode of Notation; Mr. Leibnitz calling those quantities Differences, which Mr. Newton calls Moments or Fluxions; and marking them with the letter d , a mark not used by Mr. Newton. And therefore we take the proper question to be, not who invented this or that method, but who was the first inventor of the method. And we believe, that those who have reputed Mr. Leibnitz the first inventor, knew little or nothing of his correspondence with Mr. Collins and Mr. Oldenburg long before; nor of Mr. Newton's having that method above fifteen years before Mr. Leibnitz began to publish it in the Acta Eruditorum of Lipsick.

For which reason, we reckon Mr. Newton the first inventor; and are of opinion, that Mr. Keill, in asserting the same, has been no ways injurious to Mr. Leibnitz. And we submit to the judgment of the Society, whether the extract and papers now presented to you, together with what is extant to the same purpose in Dr. Wallis's third volume, may not deserve to be made publick.

His

II. D. Leibnitium, cum primâ vice Londinum adiit, methodi ejusdem differentialis, propriè sic dictæ, se inventorem perhibuisse: et etiam si D. Pellius ipsi monstraverat, eandem antea à D. Moutono usurpatam fuisse, haud tamen sibi inventoris jura asserere destitisse; cum quia proprio, ut sicebat, Marte sua illa invenisset, nondum vilis iis quæ Moutonus prius ediderat, tum quia plurima adjecisset. Neque utquam mentionem reperimus factam alterius methodi ejus differentialis, præter istam Moutoni, ante literas ejus 21 Junii 1677 datas; hoc est, anno integro postquam D. Newtoni epistola, 10 Decembris 1672 scripta, Parisios, ipsi communicanda, transmissa fuit; & quadriennio postquam D. Collinsius eandem epistolam cum amicis communicare cœpit. In hac autem epistola methodus fluxionum, idoneo harum rerum cognitori, evidenter satis describitur.

III. Ex literis D. Newtoni, 13 Junii 1676 datis, manifestum est, fluxionum methodum ipsi innotuisse, quinque annis prius quàm epistolam illam describeret. Et ex Analysis ejus per Aequationes numero Terminorum Infinitas, quam D. Barrovius cum D. Collinio mense Julio anni 1669 communicavit, constat illum etiam ante illud tempus eandem excogitasse.

IV. Methodus Differentialis una eademque est cum Methodo Fluxionum, si nomen & notationis modum exceperis. D. Leibnitius enim eas quantitates differentias appellat, quas D. Newtonus

His autem die Aprilis 24. 1712, acceptis, Societas Regia collectionem epistolarum & MSS. & sententiam Consessûs imprimi jussit; ut & quicquid amplius, ad hanc historiam elucidandam idoneum, in Actis Eruditorum occurreret.

tonus Momenta vel Fluxiones; easque notâ literæ d designat, quam non adhibet D. Newtonus. Rem proinde de quâ agimus hanc autumamus esse; non uter hanc, uter illam methodum invenerit; sed uter methodum ipsam, quæ unica est, prior invenerit. Simul illos, qui D. Leibnitium pro inventore primo habuere, de eo, quod inter illum & D. Collinium olim intercefferat, commercio, parum aut nihil rescivisse opinamur; neque intellexisse, D. Newtonum eadem methodo usum esse, quindecim prius annos quàm D. Leibnitius eam in Actis Eruditorum Lipsiæ evulgare cœpit.

Quibus perpensis, D. Newtonum primum esse hujus methodi inventorem arbitramur; atque ideo D. Keillium, eandem illi asserendo, nullo modo D. Leibnitium calumniâ aut injuriâ affecisse. Judicio autem Societatis permittimus, utrumne excerpta literarum, reliquæque chartæ his subnexæ, unâ cum iis quæ extant in tertio volumine Operum D. Wallisii huc spectantibus, simul imprimi, & in publicum prodire mereantur.

A P P E N D I X.

HIC subungere visum est Judicium Mathematici 7 Julii 1713 datum; & chartâ volante, sine nomine auctoris, per orbem sparsum.

‘VIDETUR Newtonus occasionem nactus, serierum opus multum promovisse per extractiones radicum, quas primus in usum adhibuit; & quidem in iis excolendis, ut verisimile est, ab initio omne suum studium posuit; nec credo tunc temporis vel somniavit adhuc de calculo suo fluxionum & fluentium, vel de reductione ejus ad generales operationes analyticas, ad instar algorithmi vel regularum arithmeticarum aut algebraicarum. Ejusque meae conjecturae [primum] validissimum indicium est, quod de literis x vel y punctatis, uno, duobus, tribus, &c. punctis superpositis, quas pro dx , ddx , d^3x ; dy , ddy , &c. nunc adhibet, in omnibus istis epistolis (Commercii Epistolici, unde argumenta ducere volunt] nec volam, nec vestigium invenias. Imo ne quidem in Principiis Naturæ Mathematicis Newtoni, ubi calculo suo fluxionum utendi tam frequentem habuisset occasionem, ejus vel verbulo fit mentio, aut notam hujusmodi unicum cerne licet; sed omnia fere per lineas figurarum, sine certâ analysi, ibi peraguntur, more non ipsi tantum, sed & Hugenio, imo jam antea [in nonnullis] dudum Torricellio, Roburvallio, Cavallerio, aliis, usitato. Primâ vice hæc literæ punctatæ comparuerunt in tertio Volumine operum Wallisii, multis annis postquam calculus differentialis jam ubique locorum invaluisse. Alterum indicium, quo conjicere licet, calculum fluxionum non fuisse natum ante calculum differentialem, hoc est; quod veram rationem fluxiones fluxionum capiendi, hoc est differentiandi differentialia, Newtonus nondum cognitam habuerit; quod patet ex ipsis Principiis Phil. Math. ubi non tantum incrementum constans ipsius x , quod nunc notaret per x punctatum uno puncto, designat per o [more vulgari, qui calculi differentialis commoda destruit] sed etiam regulam circa gradus ulteriores falsam dedit [quemadmodum ab eminente quodam Mathematico dudum notatum est] Saltem apparet Newtono rectam methodum differentiandi differentialia non innotuisse, longo tempore postquam aliis fuisset familiaris.”

Hactenus Judicium.

A N N O T A T I O.

A N N O T A T I O.

HÆC omnia refutantur supra, pag. 454—458, 467—483, 489, 503, 509, 510, 512, 535, 536, 544, 581, 582, 585, 586.

Methodum fluxionum utique non consistit in formâ symbolorum. Et Keillius hoc notaverat anno 1711 (pag. 477, 585, 586.) Pro fluxionibus ipsarum x , y , z , Newtonus quandoque ponit easdem literas punctis notatas, \dot{x} , \dot{y} , \dot{z} ; quandoque easdem formâ majusculâ x , y , z ; quandoque literas alias, ut p , q , r ; quandoque lineas exponentes, ut DE , FG , HI (pag. 478.) Et hoc Newtonus in hunc usque diem facit, ut videre licet in libro de quadraturis; ubi fluxiones in propositione primâ denotantur per literas punctatas; in ultimâ, per ordinatas Curvarum; in introductione, per alia symbola; dum Newtonus ibi methodum fluxionum explicat illustratque per exempla (pag. 478.) Pro fluxionibus D. Leibnitijs nulla habet symbola. Is momentorum, sive differentiarum symbola, dx , dy , dz , primo cœpit adhibere anno 1677; Newtonus momenta denotabat per rectangula sub fluxionibus & momento o , cum analysin suam scriberet; anno scilicet 1669, aut antea. Leibnitijs symbolis \dot{x} , \dot{y} , \dot{z} pro summis ordinatarum usus est jam inde ab anno 1686: Newtonus, in analysi suâ, eandem rem denotavit, inscribendo ordinatam in quadrato vel rectangulo ad

hunc modum; $\frac{aa}{64a}$. Omnia Newtoni symbola sunt in suo genere prima; & symbola Leibnitijs

nondum obtinuerunt in Angliâ (pag. 477, 478.)

In Principiis naturæ Mathematicis Newtonus analytico suo fluxionum calculo utendi non habuit frequentem occasionem. Nam liber ille inventus est quidem per analysin; at scriptus est per synthefin, more Veterum, ut oportuit (pag. 479.) At analysi tamen ita elucet per synthefin illam, ut Leibnitijs ipse olim agnovit, Newtonum non solum methodo suâ tangentes duxisse, sed majora multo consecutum, viso demum Libro Principiorum, se satis intellexisse (pag. 489.) Et in epistolâ suâ 28 Maii 1697 ad Wallisium scripta: “Methodum, inquit, profundissimi Newtoni cognatam esse methodo meâ differentiali non tantum animadverti, postquam opus ejus [Principiorum scilicet] & tuum prodii, sed etiam professus sum in Actis Eruditorum, & aliâs quodque meum. Id enim candori meo convenire judicavi, non minus quàm ipsius merito. Itaque communi nomini designare soleo Analyticos Infinitesimales” (pag. 483.) Et alibi de sublimi quâdam parte methodi, quâ Newtonus Solidum minimæ resistentiæ invenerat, hæc habet verba. “Quam methodum, ante D. Newtonum & me, nullus quod sciam geometra habuit; uti ante hunc, maximi nominis geometram, nemo se habere PROBAVIT” (pag. 745.) Et in epistolâ ad Newtonum Hannoveræ datâ 7 Mart. 1693, ita scripsit: “Mirificè ampliaveras geometriam tuis seriebus; sed edito Principiorum opere ostendisti, patere tibi, quæ analysi receptæ non subsunt. Conatus sum ego quoque, notis commodis adhibitis, quæ differentias & summas exhibent, geometriam illam, quam transcendente appellas, analysi quodammodo subicere; nec res male successit” (pag. 472.) Atque iterum in responso ad D. Fatium, quod habetur in Actis Eruditorum Maii 1700, pag. 203, lin. 21, id fassus est Leibnitijs (pag. 472.) Sed & sectiones duas primas libri secundi Principiorum verbis aliis (absque symbolis differentialibus) composuit, & subjunxit: “se jam fundamenta geometrica jecisse, & vias quasdam novas, satis antea impeditas, aperuisse; omnia autem respondere suæ analysi infinitorum, hoc est calculo summæ ac differentiarum” (pag. 481—483.) Et hoc fuit specimen omnium primum methodi differentialis, quod D. Leibnitijs, circa problemata sublimiora orbi literario exhibuit.

Literæ punctatæ primâ vice comparuerunt, non (ut hic dicitur) in tertio volumine Operum Wallisii, quod prodiiit anno 1699; sed in secundo volumine Operum ejus, quod prodiiit anno 1693 (pag. 457, 481.) annis utique duobus antequam fama calculi differentialis ad aures Wallisii pervenerit, & annis tribus antequam Marchio Hospitalis Analysin suam infinitè parvorum eiderit, quâ calculus differentialis ubique locorum invalescere cepit (pag. 469, 473, 474, 480, 481.)

Newtonus nunquam mutavit literam o in literam x punctatam uno puncto, sed literâ illâ o usus est in introductione ad Librum de Quadraturis, & adhuc utitur in eodem sensu ac sub initio; id-

que maximo cum fructu. Est enim α symbolum unicum, quo Newtonus utitur pro quantitate infinitè parvâ: at symbolum x quantitatem finitam designat (pag. 459, 459, 477—479.)

Methodus fluxiones omnes capiendi, seu differentiandi differentialia, habetur in propositione primâ libri de quadraturis: & est verissima, & optima. Eandem, cum exemplis in differentiis primis & secundis, Wallisius edidit in tomo secundo Operum suorum anno 1693, ut suprâ; annis scilicet tribus antequam regula Leibnitii differentiandi differentialia lucem viderit (pag. 457, 480, 481.) Eandem regulam Newtonus, anno 1686, demonstravit syntheticè in Lem. II. Lib. 2. Princip. (pag. 472) & posuit in epistolâ suâ ad Oldenburgum 24 Octob. 1676, tanquam fundamentum hujus methodi, de quâ tum ante annos quinque scripserat (pag. 455.) Et specimen ejusdem, quoad tangentes ducendas, posuit in epistolâ suâ ad Collinium 10 Decemb. 1672 (pag. 510.) Et in eadem epistolâ addidit problemata de Curvarum curvaturâ, seu geometricarum seu mechanicarum, per eandem methodum solvi (pag. 510.) Ex quo manifestum est, se jam tum suam methodum ad secunda ac tertia momenta extendisse. Cum enim areæ Curvarum considerantur tanquam fluentes, ut in hac analysi fieri solet, ordinatæ exprimunt fluxiones primas, tangentes autem datæ sunt per fluxiones secundas, & Curvaturæ per tertias (pag. 456.) Et anno 1669, in analysi suâ per series, Newtonus dixit: "Momentum est superficies, cum de Solidis; & linea, cum de superficiebus; & punctum, cum "de lineis agitur:" quod perinde est, ac si dixisset: cum Solida considerantur tanquam fluentia, eorum momenta sunt superficies; & horum momentorum momenta, vel secunda Solidorum momenta, sunt lineæ; & horum momenta, sive tertia Solidorum momenta, sunt puncta; adeoque quâ ratione momenta prima derivantur à fluentibus, secunda derivantur à primis, tertia à secundis, & sic deinceps in infinitum. Et quomodo momenta prima derivantur à fluentibus, ostenditur in analysi per series; inveniendò ordinatas curvilinearum ex areis (pag. 454—458, 503.)

In eadem analysi Newtonus posuit secundam propositionem libri de quadraturis (pag. 581, lin. 30) dixitque "Curvarum areas & longitudines, id modò fiat, beneficio ejusdem methodi analytici exactè " & geometricè determinari" (p. 503, lin. 2.) Et methodus hæcce Newtono innotuit annis aliquot antea, testibus Barrovia & Collinio (pag. 509, lin. 20, 21, 22, 23, 25) id est anno 1666, aut antea. Hæc methodus aliquatenus explicatur in epistolâ Newtoni ad Oldenburgum 24 Octob. 1676 datâ; ibique ex propositione primâ libri de quadraturis, illic ænigmaticè descriptâ, consequi dicitur (pag. 535, 536) & in propositione quintâ & sextâ libri illius plenius explicatur; & hæc propositiones ex propositionibus quatuor primis libri ejusdem consequuntur: ideoque methodus fluxionum, quatenus in propositionibus quinque vel sex primis libri de quadraturis exponitur, Newtono innotuit anno 1666 aut antea, testibus Barrovia & Collinio; ut & teste etiam Wallisio (pag. 474.) Sed & Marchio Hospitalius pro Newtono testis est; qui utique dixit, librum Principiorum Philosophiæ fere totum esse ex hoc calculo (pag. 472) & Leibnitium in methodum differentialem incidisse, efficiendo, ut methodus tangentium Barrovii non haberet ad radicales (pag. 469—471) Newtonus enim, per epistolas 10 Decemb. 1672, & 24 Octob. 1676, Leibnitium admonuit, se hoc antea affectum esse (pag. 510, 544.)

Ex iis etiam, quæ in epistolâ ad Oldenburgum 24 Octob. 1676 datâ, de tabulis figurarum curvilinearum, in scholio propositionis decimæ libri de quadraturis positarum, dicuntur; liquet methodum fluxionum & momentorum, quatenus in decem primis libri illius propositionibus habetur, diu ante annum 1676 Newtono innotuisse (pag. 581.) Id quod etiam colligi potest ex Corol. 2. Prop. x. libri de quadraturis; quod utique in epistolâ Newtoni ad Collinium Nov. 8, 1676 datâ, anno 1711 à Jonesio editâ, descriptum habetur.

FINIS COMMERCII EPISTOLICI

A D D I T A M E N T A

COMMERCII EPISTOLICI

EX HISTORIA FLUXIONUM RAPHSONI.

PRÆFATUR RAPHSONUS.

*Epistola sequentes à D. Leibnitio cum amicis suis in Gallia & aliis
communicatæ, ad controversiam præcedentem spectant.*

*Prima ac tertia est ipsius Leibnitii; secunda Newtoni; omnes ad
amicum communera.*

*Prima, circa mensem Novembrem vel Decembrem anni 1715
scripta, non est epistola tota, sed Additamentum.*

“ VOILA', Monsieur, la lettre dont vous pourrez faire usage, si vous
“ le jugez à propos. Je viens maintenant à ce qui nous regarde.
“ Je suis ravi que vous estes en Angleterre; il y a dequoy profiter; & il
“ faut avouer qu'il y a là de très habiles gens; mais ils voudroient passer
“ pour être presque seuls inventeurs; & c'est in quoy apparemment ils ne
“ reussiront pas. Il ne paroist point que M. Newton ait eu avant moy la
“ caractéristique & l'algorithme infinitesimal, suivant ce que M. Ber-
“ noulli a très bien jugé: quoyqu'il luy auroit été fort aisé d'y parvenir, s'il
“ s'en fut avisé. Comme il auroit été fort aisé à Apollonius de parvenir à
“ l'Analyse de Des Cartes sur les courbes, s'il s'en étoit avisé. Ceux qui
“ ont écrit contre moy n'ayant pas fait difficulté d'attaquer ma candeur,
“ par des interpretations forcées & mal fondées; ils n'auront point le
“ plaisir de me voir répondre à de petites raisons de gens, qui en usent si
“ mal, & qui d'ailleurs s'ecartent du fait. Il s'agit du calcul des differen-
“ ces, & ils se jettent sur les series, où M. Newton m'a précédé sans dif-
“ ficulté; mais je trouvay enfin une methode generale pour les series, &
“ après cela je n'avoit plus besoin de recourir à ses extractions. Ils au-
“ roient mieux fait de donner les lettres entieres, comme M. Wallis a fait
“ avec mon consentement, & il n'a pas eu la moindre dispute avec moy,
“ comme ces gens la voudroient persuader au public. Mes adversaires
“ n'ont publié du *Commercium Epistolicum* de M. Collins, que ce qu'ils
“ ont crû capable de recevoir leur mauvaises interpretations. Je fis con-
“ noissance avec M. Collins dans mon second voyage d'Angleterre; car au
“ premier (qui dura très peu, parceque j'étois venu avec un ministre pub-
“ lic) je n'avois pas encore la moindre connoissance de la geometrie avan-
“ cée, & n'avois rien vu, ni entendu, du commerce de M. Collins avec Mess.
“ Gregory & Newton; comme mes lettres echangées avec M. Oldenbourg
“ en ce temps là, & quelque temps apres, feront assez voir. Ce n'est
“ qu'en France que j'y suis entré, & M. Hugens m'en donna l'entrée.
“ Mais à mon second voiage, M. Collins me fit voir une partie de son
“ commerce, & j'y remarquay que M. Newton avoua aussi son ignorance
“ sur plusieurs choses; & dit entre autres, qu'il n'avoit rien trouvé sur la
“ dimension

“ dimension des curvilignes celebres que la dimension de la cissoide. Mais
 “ on a supprimé tout cela. Je suis fâché qu'un aussi habile homme que
 “ M. Newton s'est attiré la censure des personnes intelligentes, en defe-
 “ rant trop aux suggestions de quelque flatteurs, qui l'ont voulu brouiller
 “ avec moy.

“ Sa philosophie me paroît un peu étrange, & je ne crois pas qu'elle
 “ puisse s'établir. Si tout corps est grave, il faut nécessairement (quoique
 “ disent ses défenseurs, & quelque emportement qu'ils temoignent) que la
 “ gravité soit une qualité occulte scholastique, ou l'effet d'un miracle.
 “ J'ay fait voir autrefois à M. Bayle, que tout ce qui n'est pas explicable par
 “ la nature des creatures est miraculeux. Il ne suffit pas de dire, Dieu à
 “ fait une telle loy de nature, donc la chose est naturelle. Il faut que la
 “ loy soit executable par les natures des creatures. Si Dieu donnoit cette
 “ loy, par exemple à un corps libre, de tourner à l'entour d'un certain
 “ centre, il faudroit ou qu'il y joignit d'autres corps, qui par leur impul-
 “ sion l'obligeassent rester toujours dans son orbite circulaire, ou qu'il mit
 “ une ange à ses trouffes; on enfin il faudroit, qu'il y concourut extraor-
 “ dinairement. Car naturellement il s'écartera par la tangente. Dieu
 “ agit continuellement sur les creatures par la conservation de leur natu-
 “ res, & cette conservation est une production continuelle de ce qui est
 “ perfection en elles. Il est *intelligentia supramundana* parce qu'il n'est
 “ pas l'ame du monde, & n'a pas besoin de *sensorium*.

“ Je ne trouve pas le vuide démontré par les raisons de M. Newton, ou
 “ de ses sectateurs; non plus que la pretendue gravité universelle, ou que
 “ les atomes. On ne peut donner dans le vuide & dans les atomes, que
 “ par des vues trop bornées. M. Clarke dispute contre le sentiment des
 “ Cartesiens, qui croient que Dieu ne sauroit détruire une partie de la ma-
 “ tiere, pour faire un vuide; mais je m'étonne qu'il ne voye point, que si
 “ l'espace est une substance différente de Dieu, la même difficulté s'y trou-
 “ ve. Or de dire que Dieu est l'espace, c'est luy donner des parties.
 “ L'espace est l'ordre des coexistences; & le temps est l'ordre des existences
 “ successives. Ce sont des choses veritables, mais ideales; comme les
 “ nombres.

“ La matiere même n'est pas une substance, mais seulement *substantia-
 tum*, un phenomene bien fondé, & qui ne trompe point, quand on y pro-
 “ cede en raisonnant suivant les loix ideales de l'arithmetique, de la geome-
 “ trie, & de la dynamique, &c. Tout ce que j'avance en cela paroît dé-
 “ montré. A propos de la dynamique, ou de la doctrine des forces, je
 “ m'étonne que M. Newton & ses sectateurs croient que Dieu a si mal
 “ fait sa machine, que s'il n'y mettoit la main extraordinairement, la
 “ montre cesseroit bien tôt d'aller. C'est d'avoir des idées bien étroites de
 “ la sagesse & de la puissance de Dieu. J'appelle extraordinaire toute ope-
 “ ration de Dieu, qui demande autre chose que la conservation des natures
 “ des creatures. Ainsi quoy que je croye la metaphysique de ces messieurs
 “ là, a narrow one, & leur mathematique assez arrivable; je ne laisse pas
 “ d'estimer extremement les meditations physico-mathematiques de M.

“ Newton;

“ Newton; & vous obligeriez infiniment le public, monsieur, si vous
 “ portiez cet habile homme à nous donner jusqu'à ses conjectures en phy-
 “ sique. J'approuve fort sa methode de tirer des phenomènes, ce qu'on en
 “ peut tirer sans rien supposer, quand même ce ne seroit quelquefois que
 “ tirer des consequences conjecturales. Cependant quand les *data* ne suf-
 “ fisent point, il est permis (comme on fait quelquefois en déchiffrant)
 “ d'imaginer des hypotheses; & si elles sont heureuses, on s'y tient provi-
 “ sionnellement, en attendant que des nouvelles experiences nous appor-
 “ tent *nova data*, & ce que Bacon appelle *Experimenta Crucis*, pour choisir
 “ entre les hypotheses. Comme j'apprends que certain Anglois ont mal
 “ représenté ma philosophie dans leur Transactions, je ne doute point qu'
 “ avec ce que je vous m'ande icy, je ne puisse être justifié. Je suis fort
 “ pour la philosophie experimentale, mais M. Newton s'en ecarte fort,
 “ quand il prétend que toute la matiere est pesante, ou que chaque partie
 “ de la matiere attire chaque autre partie; ce que les experiences ne prou-
 “ vent nullement; comme M. Hugen à déjà fort bien jugé, la matiere
 “ gravifique ne sauroit avoir elle même cette pesanteur, dont elle est la
 “ cause, & M. Newton n'apporte aucune experience, ni raison suffisante, pour
 “ le vuide & les atomes, ou pour l'attraction mutuelle generale. Et parce
 “ qu'on ne fait pas encore parfaitement, & en detail, comment se produit
 “ la gravité, ou la force elastique, ou la magnetique, &c; on n'a pas rai-
 “ son pour cela d'en faire des qualitez occultes scholastiques, ou des mira-
 “ cles; mais on à encore moins raison de donner des bornes à la sagesse &
 “ à la puissance de Dieu, & de luy attribuer un *sensorium* & choses sembla-
 “ bles. Au reste, je m'étonne que les sectateurs de M. Newton ne don-
 “ nent rien, qui marque que leur maître leur à communiqué une bonne
 “ methode j'ay été plus heureux en disciples.”

Leicester-Fields,

S I R,

London; Feb. 26, 1713.

YOU know that the *Commercium Epistolicum* contains the antient letters
 and papers preserved in the Archives and letter-books of the Royal So-
 ciety, and library of Mr. Collins, relating to the dispute between Mr. Leib-
 nitz and doctor Keill; and that they were collected and published by a
 numerous committee of gentlemen of several nations, appointed by the
 Royal Society for that purpose. Mr. Leibnitz has hitherto avoided return-
 ing an answer to the same; for the book is matter of fact, and incapable
 of an answer. To avoid answering it, he pretended the first year that he
 had not seen this book, nor had leisure to examine it, but had desired an
 eminent mathematician to examine it. And the answer of the mathema-
 tician (or pretended mathematician) dated June 7, 1713, was inserted in-
 to a defamatory letter dated July 29 following, and published in Germany
 without the name of the author or printer, or city where it was printed.
 And the whole has been since translated into French, and inserted into an-
 other

other abusive letter (of the same author, as I suppose) and answered by Dr. Keill in July 1714; and no answer is yet given to the doctor.

Hitherto Mr. Leibnitz avoided returning an answer to the *Commercium Epistolicum*, by pretending that he had not seen it. And now he avoids it, by telling you, that the English shall not have the pleasure to see him return an answer to their slender reasonings (as he calls them) and by endeavouring to engage me in disputes about Philosophy, and about solving of Problems; both which are nothing to the question.

As for Philosophy, he colludes in the significations of words, calling those things miracles, which create no wonder; and those things occult qualities, whose causes are occult, though the qualities themselves be manifest; and those things the souls of men, which do not animate their bodies. His *Harmonia Præstabilita* is miraculous, and contradicts the daily experience of all mankind; every man finding in himself a power of seeing with his eyes, and moving his body by his will. He prefers hypotheses to arguments of induction drawn from experiments; accuses me of opinions which are not mine; and instead of proposing questions to be examined by experiments before they are admitted into Philosophy, he proposes hypotheses to be admitted and believed, before they are examined: but all this is nothing to the *Commercium Epistolicum*.

He complains of the committee of the Royal Society, as if they had acted partially in omitting what made against me; but he fails in proving the accusation. For he instances in a paragraph concerning my ignorance, pretending that they omitted it, and yet you will find it in the *Commercium Epistolicum*, pag. 547, lin. 2, 3, and I am not ashamed of it. He saith, that he saw this paragraph in the hands of Mr. Collins when he was in London the second time; that is, in October 1676. It is in my letter of the 24th of Octob. 1676, and therefore he then saw that letter. And in that, and some other letters writ before that time, I described my method of fluxions. And in the same letter I described also two general methods of series, one of which is now claimed from me by Mr. Leibnitz.

I believe you will think it reasonable, that Mr. Leibnitz be constant to himself; and still acknowledge what he acknowledged above 15 years ago; and still forbear to contradict what he forbore to contradict in those days.

In his letter of the 20th of May 1675, he acknowledged the receipt of a letter from Mr. Oldenburg, dated the 15th of April 1675, with several converging series contained therein. And I expect from him, that he still acknowledge the receipt thereof. Many gentlemen of Italy, France, and Germany (yourself being one of them) have seen the original letters, and the entries thereof in the old letter-books of the Royal Society; and the Series of Gregory is in the letter of the 15th of April 1675, and in Gregory's original letter, dated the 15th of Feb. 1671.

In a letter dated the 12th of May 1676 (seen by the same gentlemen) he acknowledged that he then wanted the method for finding a Series for the arc whose sine was given; and, by consequence, that he wanted it when

he

he wrote his letter of the 24th of October 1674; and I expect that he still acknowledge it.

In the *Acta Eruditorum* for May 1700, in answer to Mr. Fatio, who had said, that I was the oldest inventor by many years, Mr. Leibnitz acknowledged that nobody, so far as he knew, had the method of fluxions or differences before me and him; and that nobody before me had proved, by a specimen made publick, that he had it. Here he allowed, that I had the method before it was published, or communicated by him to any body in Germany; that the *Principia Philosophiæ* were a proof that I had it, and the first specimen made publick of applying it to the different problems: and I expect, that he still continue to make the same acknowledgment. At that time he did not deny what Mr. Fatio affirmed, and nothing but want of candour can make him unconstant to himself.

In a letter to me dated the 7th of March 1693, and now in the custody of the Royal Society, he wrote, *Mirissè amplius veras geometriam tuis scribis; sed edito Principiorum opere, ostendisti patere tibi, quæ analysi receptæ non subsunt. Conatus sum ego quoque, notis commodis adhibitis, quæ differentias & summas exhibent, geometriam illam quam transcendentem appello, analysi quodammodo subicere, nec res malè processit.* And what he then acknowledged, he ought still to acknowledge.

In his letter of the 21st of June 1677, writ in answer to mine of the 24th of October 1676, wherein I described my method partly in plain words, and partly in cyphers; he said, that he agreed with me, that the method of tangents of Slusius was not yet made perfect; and then set down a Differential Method of Tangents, published by Dr. Barrow in the year 1670, and disguised it by a new notation, pretending that it was his own, and shewed how it might be improved, so as to perform those things which I had ascribed to my method; and concluded from thence, that mine differed not much from his, especially since it facilitated quadratures. And in the *Acta Eruditorum* for Octob. 1684, in publishing the Elements of his Method, he added, that it extended to the difficulter problems, which without this method, or another like it, could not be managed so easily. He understood therefore in those days, that in the year 1676, when I wrote my said letter, I had a method which did the same things with the method which he calls Differential, and he ought still to acknowledge it; especially now the sentences in cyphers are decyphered, and other things in that letter, relating to the method, are fully explained, and the Compendium mentioned therein made publick.

In his letter of the 27th of August 1676, he represented, that he did not believe that my methods were so general as I described them in my letter of the 13th of June preceding; and affirmed, that there were many problems so difficult, that they did not depend upon equations or quadratures, such as (amongst many others) were the inverse problems of tangents. And by these words he acknowledged, that he had not yet found the reduction of problems to Differential Equations. And what he then

acknow-

acknowledged, he acknowledged again in the *Acta Eruditorum* for April 1691, pag. 178, and ought in candour to acknowledge still.

Doctor Wallis, in the preface to the two first volumes of his works, published in April 1695, wrote, that I, in my two letters written in the year 1676, had explained to Mr. Leibnitz the method (called by me, the Method of Fluxions, and by him, the Differential Method) invented by me ten years before, or above (that is, in the year 1666, or before): and in the letters which followed between them, Mr. Leibnitz had notice of this paragraph, and did not then contradict it; nor found any fault with it; and I expect that he still forbear to contradict it.

But as he has lately attacked me with an accusation which amounts to plagiarism; if he goes on to accuse me, it lies upon him by the laws of all nations to prove his accusations, on pain of being accounted guilty of calumny. He hath hitherto written letters to his correspondents full of assertions, complaints and reflections, without proving any thing. But he is the aggressor, and it lies upon him to prove the charge.

I forbear to descend further into particulars; you have them in the *Commercium Epistolicum*, and the abstract thereof, to both which I refer you. I am, Sir, yours, &c.

MONSIEUR,

C'EST sans doute pour l'amour de la verité, que vous vous etes chargé d'une espace de cartel de la part de M. Newton. Je n'ay point voulu entrer en lice avec des enfans perdus, qu'il avoit detachés contre moy; soit qu'on entende celui, qui a fait l'accusateur sur le fondement du *Commercium Epistolicum*; soit qu'on regarde la preface pleine d'aigreur, qu'un autre a mise devant la nouvelle edition de ses principes. Mais puisqu'il veut bien paroître luy même, je seray bien aise de luy donner satisfaction.

Je fus surpris au commencement de cette dispute d'apprendre qu'on m'accusoit d'être l'agresseur; car je ne me souvenois pas d'avoir parlé de M. N. que d'une maniere fort obligeante. Mais je vis depuis, qu'on abusoit pour cela d'un passage des Actes de Leipzig du Janvier 1703, ou il y a ces mots: *Pro differentiis L...ianis, D. N...nus adhibet, semperque adhibuit fluxiones*; ou l'auteur des remarques sur le *Commercium Epistolicum* dit, pag. 577, *Sensus verborum est, quod N...nus fluxiones differentiis L...tianis substituit*. Mais c'est une interpretation maligne d'un homme qui cherchoit noise: il semble que l'auteur de paroles inséré dans les Actes de Leipzig a voulu y obvier tout expres, par ces mots, *adhibet semperque adhibuit*; pour insinuer, que ce n'est pas apres la veue de mes differences, mais deja auparavant, qu'il s'est servi de fluxions. Et je desie, qui que ce soit, de donner un autre but raisonnable à ces paroles, *semperque adhibuit*. Au lieu qu'on se sert du mot *substituit*, en parlant de ce que le Pere Fabri avoit fait apres Cavallieri. D'où il faut conclure on

que M. N. s'est laissé tromper, par un homme qui a empoisonné ces paroles des Actes, qu'on supposoit n'avoir pas été publiées sans ma connoissance, & s'est imaginé qu'on l'accusoit d'être plagiaire; ou bien qu'il a été bien aise de trouver un pretexte, de s'attribuer ou faire attribuer privément l'invention du nouveau calcul (depuis qu'il en remarquoit le succès, & le bruit qu'il faisoit dans le monde) contre ses connoissances contraires avouées dans son livre des Principes, pag. 253, de la premiere edition. Si l'on avoit fait connoître qu'on trouvoit quelque difficulté, ou sujet de plainte, dans les paroles des Actes de Leipzig; je suis assuré, que ces messieurs, qui ont part à ces Actes, auroient donné un plein contentement; mais il semble qu'on cherchoit un pretexte de rupture.

Je n'ay pas eu connoissance du *numerous committee of gentlemen of several nations relating to the dispute*; car on ne m'en a donné aucune part, & je ne say pas encore presentement les noms de tous ces commissaires, & particulierement de ceux qui ne sont pas des isles Britanniques: je ne crois pas qu'ils approuvent tout ce qui a été mis dans l'ouvrage publié contre moi.

Il est aisé à croire que j'ay été quelque temps à Vienne, avant que d'avoir vu le *Commercium Epistolicum* déjà publié, quoique j'en eusse des nouvelles. Ainsi un ami sachant cela, aussi zélé pour moi que les seconds de M. N. le peuvent être pour luy, a publié un papier, que M. N. appelle diffamatoire (*defamatory letter*.) Mais cette piece n'étant pas plus forte, que ce qu'on a publié contre moi, M. N. n'a pas droit de s'en plaindre. Si l'on n'a pas marqué l'auteur ni le lieu de l'impression du papier; on connoît assez le nom & le lieu de l'auteur de la lettre y insérée d'un excellent mathématicien, que j'avois prié de dire son sentiment sur le *Commercium*, & cela suffit. M. N. (dont les partisans ont marqué qu'il ne leur étoit pas inconnu) l'appelle un mathématicien, ou prétendu mathématicien; & apres avoir fait inutilement des efforts pour le gagner, il le méprise contre l'opinion publique, qui le met entre ceux du premier rang, & contre l'evidence des choses vérifiées par ses découvertes.

Lors que j'eus enfin le *Commercium Epistolicum*, je vis qu'on s'y écartoit entierement du but; & que les lettres, qu'on publioit, ne contenoient pas un mot, qui peut faire revoquer en doute mon invention du calcul des differences, dont il s'agissoit. Au lieu de cela je remarquay qu'on se jettoit sur les series; ou l'on accorde l'avantage à M. N. & que les remarques contenoient de gloses mal tournées, pour tacher de me decrier par des soupçons sans fondement quelquefois ridicules, & quelquefois forgés, contre la conscience de quelques uns de ceux qui en étoient les auteurs, ou approbateurs.

Pour repondre donc de point en point à l'ouvrage publié contre moi, il falloit un autre ouvrage aussi grand pour le moins que celui là; il falloit entrer dans un grand detail de quantité de minuties passées il y a 30 ou 40 ans dont je ne me souvenois gueres; il me falloit chercher mes vieilles lettres, dont plusieurs se sont perdues, outre que le plus souvent, je n'ay pas gardé les minutes des miennes; & les autres sont ensevelies dans un grand

grand tas de papiers, qui je ne pouvois debrouiller qu'avec du temps & de la patience. Mais je n'en avois gueres le loisir, étant chargé presentement d'occupations d'une toute autre nature.

De plus je remarquay, que dans la publication du *Commercium Epistolicum* on a supprimé des endroits qui pouvoient être au desavantage de M. N.; au lieu qu'on n'y a rien omis, de ce qu'on croyoit pouvoir tourner contre moi par des gloses forcées. Comme je n'ay pas daigné lire le *Commercium Epistolicum* avec beaucoup d'attention, je me suis trompé dans l'exemple que j'ay cité, n'ayant pas pris garde, ou ayant oublié qu'il s'y trouvoit; mais j'en citeray un autre. M. N. avouoit dans un des ses lettres à M. Collins, qu'il ne pouvoit point venir à bout des sections secondes (ou segments seconds) de spheroides, ou corps semblables: mais on n'a point inieré ce passage, ou cette lettre, dans le *Commercium Epistolicum*; il auroit été plus sincere par rapport à la dispute, & plus utile au public, de donner le commerce litteraire de M. Collins tout entier, là ou il contenoit quelque chose qui meritoit d'être lû; & particulierement de ne pas tronquer les lettres; car il y en a peu parmi mes papiers, ou dont il me reste des minutes.

Ainsi tout considéré, voyant tant de marques de malignité & de chicane, je crûs indigne de moi d'entrer en discussion avec des gens, qui en usoient si mal. Je voyois qu'en les refutant on auroit de la peine à éviter des reproches, & des expressions fortes, telles que meritoit leur procedé; & je n'avois point envie de donner ce spectacle au public, ayant dessein de mieux employer mon temps, qui me doit être precieux, & meprisant assez le jugement de ceux, qui sur un tel ouvrage voudroient prononcer contre moi, d'autant que la Société Royale même ne la point voulu faire; comme je l'ay appris par un extrait de ses registres.

Je ne crois point d'avoir dit (comme M. N. me l'impute) que les Anglois n'auroient point le plaisir de me voir repondre à des raisonnemens si minces; car je ne crois point que tous les Anglois fassent leur cause de celle de M. N. il y en a de trop habiles & de trop honnêtes pour épouser les passions de quelquesuns de ses adherens.

Après cela, il m'accuse d'avoir voulu faire diversion, en combattant sa philosophie, & en voulant l'engager dans des problemes; mais quant à la philosophie, j'ay donné publiquement quelque chose de mes principes sans attaquer les siens; si ce n'est que par occasion j'en ay parlé dans des lettres particulieres, depuis qu'on m'en a donné sujet; & pour ce qui est des problemes, je n'ay garde d'en proposer à M. N. car je ne voudrois pas m'y engager, quand on m'en proposeroit à moi: nous pouvons nous en dispenser à l'age ou nous sommes; mais nous avons des amis, qui y peuvent suppléer à notre défaut.

Je ne veux point entrer icy dans le detail de ce que M. N. dit un peu aigrement contre ma philosophie; car pour la sienne, ce n'en est point le lieu. J'appelle Miracle tous evenement qui ne peut être arrivé que par la puissance de Createur, sa raison n'étant pas dans la nature des creatures; & quand on veut neanmoins l'attribuer aux qualités ou forces des creatures; alors

alors j'appelle cette qualité une qualité occulte à la scholastique; c'est à dire, qu'il est impossible de rendre manifeste: telle que seroit une pesanteur primitive; car les qualités occultes qui ne sont point chimeriques, sont celles dont nous ignorons la cause, mais que nous n'excluons point; & j'appelle l'ame de l'homme cette substance simple, qui s'apperçoit de se qui se passe dans le corps humain, & dont les appetits ou volontés sont suivis par les efforts du corps. Je ne préfère pas les hypotheses aux arguments tirez de l'induction des experiences, mais quelquefois on fait passer pour inductions generales, ce qui ne consiste qu'en observations particulieres, & quelquefois on veut faire passer pour une hypothese ce qui est demonstratif. L'idée que M. N. donne icy de mon harmonie preetablie n'est pas celle qu'en ont quantité d'habiles gens hors d'Angleterre, & quelques uns en Angleterre; & je ne crois pas que vous même Monf. en ayez eu une semblable, ou l'ayez maintenant, à moins que d'être bien changé.

Je n'ay jamais nié, qu'à mon second voyage en Angleterre j'aye vû quelques lettres de M. N. chez Monsieur Collins; mais je n'en ay jamais vû, ou M. N. ait expliqué sa methode des fluxions, & je n'en trouve point dans le *Commercium Epistolicum*.

Je n'ay pas vû non plus qu'il ait expliqué la methode des series que je m'attribue; je crois qu'il veut parler de celle ou je prends une series arbitraire; je l'ay fait avant mon second retour en Angleterre. Je ne nie pourtant pas, que M. N. n'eut pû l'avoir aussi, & ce n'est pas même une invention fort difficile.

M. N. veut que j'avoue, & que j'accorde ce que j'ay avoué ou accordé il y a 15 ans. Ou autrement on devroit en attendre de luy autant; car il y a maintenant deux fois quinze ans, que dans la premiere edition de ses Principes, pag. 253, 254, il m'accorde l'invention du calcul des differences, independamment de la sienne; & depuis il s'est avisé, je ne scay comment, de faire soutenir le contraire.

Il est bon de sçavoir, qu'à mon premier voyage d'Angleterre en 1673, je n'avois pas la moindre connoissance des series infinies, telles que M. Mercator venoit de donner; ny d'autres matieres de la geometrie avancée par les dernieres methodes: je n'étois pas même assez versé dans l'analyse de Des Cartes; je ne traitois les mathematiques que comme un Parergon; & je ne savois guere que la geometrie pratique vulgaire, quoy que j'eusse vû par hazard la geometrie des indivisibles de Cavalleri, & un livre de Pere Leotaud, ou il donnoit les quadratures des lunules & figures semblables, ce qui m'avois donné quelque curiosité; mais je me divertissois plutôt aux proprietés des nombres, à quoy le petit traité que j'avois publié, presque petit garçon, de l'art des combinaisons en 1666 m'avoit donné occasion; & ayant observé des lors l'usage des differences pour les sommes, je l'appliquay à des suites de nombres. On voit bien par mes premieres lettres echangées avec M. Oldenbourg, que je n'étois guere allé plus avant, aussi n'avois je point alors la connoissance de M. Collins, quoy qu'on ait feint malicieusement le contraire.

‘ Ce fût peu à peu que M. Hugen me fit entrer en ces matieres, quand je le pratiquois à Paris, & cela joint au traité de M. Mercator (que j’avois rapporté avec moi d’Angleterre parce que M. Pell m’en avoit parlé) me fit trouver environ, vers la fin de l’an 1673, ma quadrature arithmetique du cercle, qui fût fort approuvé par M. Hugen, & dont je parlay à M. Oldenbourg dans une lettre de l’an 1674 : alors ny M. Hugen ny moi, nous ne favions rien des series de M. N. ny de M. Gregory. Ainsi je crus être le premier, qui eut donné la valeur du cercle par une suite de nombres racionales, & M. Hugen le crut aussi ; j’en écrivis sur ce ton-là à M. Oldenbourg, qui me repondit qu’on avoit déjà de telles series en Angleterre ; & l’on voit par ma lettre du 15 Juillet de 1674, & par la réponse de M. Oldenbourg du 8 Decembre de la même année, que je n’en devois avoir aucune connoissance alors ; autrement M. Oldenbourg n’auroit pas manqué de me le faire sentir, si luy ou M. Collins m’en eussent communiqué quelque chose ; mais je ne savois pas alors les extractions des racines des equations par des series, ni les regreffions ou l’extraction d’une equation infinie ; j’étois encore un peu neuf en ces matieres ; mais je trouvai pourtant bientôt ma methode generale par des series arbitraires ; & j’entrai enfin dans mon calcul des differences, ou les observations que j’avois faites, encore fort jeune, sur les differences des suites des nombres, contribuerent à m’ouvrir les yeux ; car ce n’est pas par les fluxions des lignes, mais par les differences des nombres que j’y suis venu, en considerant enfin que ces differences appliquées aux grandeurs qui croissent continuellement, s’évanouissent en comparaison des grandeurs différentes, au lieu qu’elles subsistent dans les suites des nombres. Et je crois que cette voye est la plus analytique ; le calcul geometrique des differences qui est le même que celui des fluxions, n’étant qu’un cas special, devient plus commode par les evanouissements.

‘ M. N. allegue par apres les passages, ou j’accorde, qu’il y a un calcul approchant de mon calcul des differences ; mais il pourra bien se souvenir qu’il m’en a accordé autant, et s’il luy est permis de se retracter, pourquoy ne me fera t’il pas permis d’en faire autant, sur tout apres les verisimilitudes que M. Bernouilli a remarquées ? J’ay une si grande opinion de la candeur de M. N. que je l’ay crû sur sa parole ; mais le voyant conriver à des accusations dont la fausseté luy est connue, il étoit naturel que je commençasse de douter.

‘ Je ne puis avouer ny desavouer aujourd’huy d’avoir écrit, ou reçu des lettres écrites, il y a plus de 40 ans telles qu’on les a publiées ; je suis obligé de m’en rapporter à ce qui se trouve dans les papiers qu’on cite, mais je ne remarque rien contre moi dans celles que M. N. allegue du 15 Avril & 20 May 1675, & du 24 Octobre 1676, sinon dans les faussetez du Glossateur. Je crois que c’étoit purement par distraction, dans un séjour comme celui de Paris, ou je m’occupois à bien d’autres choses encore qu’aux mathematiques, & par l’éloignement que j’avois des calculs, dont je craignois la longueur, que j’ay demandé quelquefois à M. Oldenbourg la demonstration ou la methode d’arriver à certaines choses, ou j’aurois bien

‘ bien pû arriver moi même. Par exemple, je crois d’avoir déjà eu au douze de May 1676, ma methode d’une series arbitraire, qui m’auroit pû mener à des series, dont j’y demande la raison. Car ayant consulté mon vieux traité de la Quadrature Arithmetique, achevé quelque temps avant ma sortie de France, je m’y fers de la series arbitraire ; cependant les series marquées dans cette lettre sont une chose dont je consens d’être redevable à d’autres, & je crois de ne les avoir pas même connues en 1674.

‘ N’entendant pas bien ce que M. N. allegue des Actes de Leipzig de May 1700, j’y ay regardé, & je trouve qu’il n’en a pas bien pris le sens. Il n’y est point parlé de l’invention du nouveau calcul des differences, mais d’un artifice particulier des *Maximis & Minimis*, qui en est indépendant, & dont je m’étois avisé bien du temps avant que M. Bernouilli eut proposé son probleme de la plus courte descende, mais dont je jugeois que M. N. se devoit être avisé aussi, lors qu’il avoit donné la figure de son vaisseau dans ses principes. Ainsi j’ay voulu dire, qu’il a fait connoître publiquement avant moi, qu’il possédoit cet artifice ; ce que je ne pouvois pas dire du calcul des differences & des fluxions, puisque j’en avois fait voir l’utilité publiquement avant la publication de ce livre. Cet artifice particulier de *Maximis & Minimis* n’est point nécessaire, quand ils s’agit simplement d’une grandeur (car alors la methode de M. Fermat perfectionnée par les nouveaux calculs suffit) mais quand il s’agit de toute une figure qui doit faire le mieux un effet demandé, il faut autre chose.

‘ M. N. hazarde icy une accusation mais, qui va tomber sur luy même. Il pretend que ce que j’ay écrit pour luy à M. Oldenbourg en 1677, est un deguisement de la methode de M. Barrow. Mais comme M. N. avoue dans la pag. 253 & 254, de la premiere edition de ses Principes, *Me ipsi tunc methodum communicasse à methodo ipsius vix abludentem præterquam in verborum & notarum formulis*, il s’enfuivra, que sa methode aussi n’est qu’un deguisement de celle de M. Barrow.

‘ Je croy que luy & moi nous ferons aisément quites de cette accusation : car une infinité de gens liront de livre de M. Barrow, sans y trouver nôtre calcul ; il est vray que feu M. Tschirnhaus, qui s’apperçut un peu tard de l’avantage de ce calcul, pretendoit qu’on pouvoit arriver à tout cela par les methodes de M. Barrow. Comme l’Abbé Catelan Francois pretendit que même l’Analyse de Des Cartes suffisoit pour toute ces choses ; mais il étoit plus aisé de le dire, que de le montrer.

‘ Cependant si quelqu’un a profité de M. Barrow, ce sera plus tôt M. N. qui a étudié sous luy, que moi qui (autant que je puis m’en souvenir,) n’ay veu les livres de M. Barrow qu’à mon second voyage d’Angleterre, & ne les ay jamais lûs avec attention ; parce qu’en voyant le livre, je m’apperçus que par la consideration du triangle caracteristique (dont les cotez sont les elements de l’abscisse, de l’ordonnée & de la courbe) semblable à quelque triangle assignable, j’étois venu, comme en me jouant, aux quadratures, surfaces, & solides, dont M. Barrow avoit remplis un chapitre des plus considerables de ses leçons ; outre que je ne suis venu à mon calcul des differences dans la geometrie, qu’apres en avoir vu l’usage (mais moins.

moins confiderable) dans les nombres, comme mes premières lettres dans le *Commercium Epistolicum* le peuvent infirmer. Il se peut que M. Barrow en ait plus scu, qu'il n'a pas dit dans son livre, & qu'il a donné des lumières à M. N. que nous ne favons pas : & si j'étois semblable à certains teméraires, je pourrois affeurer sur de simples soupçons, sans autre fondement, que le calcul des fluxions de M. N. qu'il qu'il puisse être, luy a été enseigné par M. Barrow.

On peut bien juger que lors que j'ay parlé en 1676, des problèmes qui ne dependoient, ni des equations, ni des quadratures, j'ay voulu parler des equations telles qu'on connoissoit alors dans le monde ; c'est à dire, des equations de l'analyse ordinaire. Et on le peut juger, de ce que j'ajoute les quadratures comme quelque chose de plus que ces equations. Mais les equations différentielles vont au delà même des quadratures ; & l'on voit bien que j'entendois même parler des problèmes, qui vont à ces fortes d'equations inconnues alors au public ; cette objection se trouvoit déjà dans les remarques au *Commercium*, mais je n'avois point cru que M. N. étoit capable de l'employer.

Je juge par un endroit de ma lettre du 27 d'Aoust 1676 (*vid. pag. 539, hujus Tomi*) que je devois déjà avoir alors l'ouverture du calcul des différences ; car j'ay dit d'avoir résolu d'abord par une certaine analyse (*certâ Analyfi solvi*) le problème de M. de Beaune proposé à M. Des Cartes ; si cette analyse n'étoit que cela, on le peut résoudre sans cela ; & je crois que Monsieur Hugens & Monsieur Barrow l'auroient donné au besoin, comme beaucoup d'autres choses : mais selon ma manière de noter, ce n'est qu'un jeu ; je trouve une petite faute dans cette page, il y a *ludus naturæ* au lieu de *lufus naturæ*, mais cette faute étoit ancienne, & se devoit déjà trouver dans la copie de ma lettre du 24 d'Octobre (1676, *pag. 539, hujus Tomi*.) *Hos casus vix numeraverim inter lufus naturæ.* Je n'avois point entendu ce qu'il vouloit dire, mais à présent je vois l'origine de la méprise.

Je ne saurois dire aujourd'hui, si j'ay remarqué le passage de M. Wallis, ou il dit que M. N. servoit déjà la méthode des fluxions en 1666. Mais quand je l'aurois remarqué, je l'aurois laissé passer apparemment, étant fort porté alors à croire M. N. sur sa parole. Mais son dernier procédé m'a forcé d'être plus circonspect à cet égard.

M. N. dit, que je l'ay accusé d'être plagiaire : mais ou est ce que je l'ay fait ? Ce sont ses adhérens, qui ont paru intenter cette accusation contre moi, & il y a connivé. Je ne say pas s'il adopte entièrement ce qu'ils ont publié ; mais je conviens avec luy, que la malice de celui qui intente une telle accusation sans la prouver, le rend coupable de calomnie.

Il finit sa lettre en m'accusant d'être l'agresseur, & j'ay commencé celle cy en prouvant le contraire. Il sera fort aisé de vider ce point préliminaire. Il y a eu du méfentendu, mais ce n'est pas ma faute ; au reste je suis avec zèle.

Votre tres humble & tres obeissant serviteur,

LEIBNITZ.

Hannover,
ce 9 d'Avril 1716.

Cum D. Leibnitius adduci non posset, ut vel Commercio Epistolico responderet, vel probaret quæ pro lubitu affirmabat, cumque præcedentes epistolas in Galliam prius mitteret, quàm earum tertia in Angliam veniret, & prætenderet se hoc facere, ut testes haberet, & alias etiam adhiberet contumelias : Newtonus minimè rescriptis, sed observationes sequentes in epistolam illam tertiam scriptas, cum amicis solummodo communicavit.

Observations upon the preceding EPISTLE.

MR. LEIBNITZ, by his letter of the 29th of December 1711, justified the passage in the *Acta Eruditorum* for January 1705, pag. 34 and 35, and thereby made it his own, and now endeavours in vain to excuse it, pretending that the words *adhibet semperque adhibuit* are maliciously interpreted by the word *substituit*. But in the interpretation which he would put upon the place, he omits the words *igitur* and *quemadmodum* ; the first of which makes the words, *semperque adhibuit*, a consequence of what went before, and the latter makes them equivalent to *substituit* ; neither of which can be true, in the sense which Mr. Leibnitz endeavours now to put upon the words. He has therefore accused me. In both his letters to Dr. Sloane (that dated the 4th of March, and that dated the 29th of December 1711) he pressed the Royal Society to condemn Dr. Keill ; and before I meddled in this matter, challenged me to declare my opinion. His words in his second letter are : *Itaque vestrae equitati committo, annon coercenda sint vane & injustæ [Keill] vociferationes, quas ipsi Newtono viro insigni, & gestoribus optimè conscio, improbari arbitror ; ejusque sententiæ suæ libenter daturum indicia mihi persuadeo.* The words are civil, but the sense is, That I must either condemn Dr. Keill, or enter into a quarrel with Mr. Leibnitz ; as has happened : and therefore he is the aggressor. For it is very well known here, that I constantly endeavoured to avoid these disputes, till they were pressed upon the Royal Society and me.

In his letter of the 4th of March st. n. 1711, he pressed the Royal Society to condemn Dr. Keill, without hearing both parties ; and when the Doctor put in his answer, Mr. Leibnitz refused to give his reasons against the Doctor, and called it injustice to expect it from him, and yet persisted in pressing them against him, and thereby put them upon a necessity of appointing a committee to search out old papers, and give their opinion upon them. If they did it without him, it was his own fault : he was for over-ruling them, and called it injustice to expect that he should defend his candor, and plead before them. If they gave him no opportunity to except against any of the committee ; it was because he refused to be heard : and they had a sufficient authority to appoint a committee without him, and he had no right to except against what they did for their own satisfaction. If they have not yet given judgment against him ; it is because the committee did not act as a jury, nor the Royal Society as a formal court of justice. The committee examined old letters and papers, and gave their

their opinion upon them alone, and left room for Mr. Leibnitz to produce further evidence for himself. And it is sufficient that the Society ordered their report, with the papers upon which it was grounded, to be published; and that Mr. Leibnitz, in all the three years and four months which are since elapsed, has not been able to produce any further proof against Dr. Keill, than what was then before them.

Mr. Leibnitz saith, That the letter which I call defamatory, being no sharper than that which has been published against him, I have no reason to complain. But the sharpness of the letter lies in accusations and reflections, without any proof; which way of writing is unlawful and infamous, and never used but in a bad cause. The sharpness of the *Commercium* lies in facts, which are lawful and fit to be produced. The letter was published in a clandestine, back-biting manner (as defamatory papers use to be) without the name of the author, or mathematician, or printer, or city where it was printed; and was dispersed above two years, before we were told that the mathematician was John Bernouilli; the *Commercium* was printed openly at London by order of the Royal Society.

The mathematician to whom Mr. Leibnitz appealed from the Royal Society, I called a mathematician, or pretended mathematician, not to disparage the skill of Mr. Bernouilli; but because the mathematician, in his letter of the 7th of June 1673, cited Mr. Bernouilli as a person distinct from himself; and Mr. Leibnitz lately caused that letter to be reprinted without the citation; and tells us, that the mathematician was Mr. Bernouilli himself: and whether the mathematician or Mr. Leibnitz is to be believed, I do not know. Mr. Bernouilli had the Differential Method from Mr. Leibnitz, and was the chief of his disciples, and gave his opinion for his master in the *Acta Lipsica* before he saw the *Commercium Epistolicum*; at which time he was *homo novus, & rerum anteaëtarum parum peritus*, as Mr. Leibnitz objected against Dr. Keill; and what he wrote after he saw the *Commercium* was in his own defence, and his skill in mathematicks will not mend the matter.

He complains, that the committee had gone out of the way, in falling upon the Method of Series. But he should consider, that both methods are but two branches of one general method of Series: I joined them together in my Analysis; I interwove them in the tract which I wrote in the year 1671, as I said in my letters of the 10th of December 1672, and the 24th of October 1676. In my letter of the 13th of June 1676, I said, that my method of Series extended to almost all problems, but became not general without some other methods, meaning (as I said in my next letter) the method of Fluxions, and the method of arbitrary Series: and now to take those other methods from me, is to restrain and stint the method of Series, and make it cease to be general. In my letter of the 24th of October 1676, I called all these methods together, my General Method. See pag. 547, lin. 20. And if Mr. Leibnitz has been tearing this general method in pieces, and taking from me first one part, and then another part, whereby the rest is maimed, he

has given a just occasion to the committee to consider the whole. It is also to be observed, that he is perpetually giving testimony for himself; and it is allowed, in all courts of justice, to speak to the credit of the witness.

He represents, that the committee of the Royal Society have omitted things which made against me, and printed every thing which could be turned against him by strained glosses; and to make this appear, he produces in his last letter but one, an instance of my ignorance omitted by them; but confesses now, that he was mistaken in saying that it was omitted, and saith that he will cite another instance. He saith, that in one of my letters to Mr. Collins, I owned that I could not find the second Segments of Sphaeroids, and that the committee have omitted this. If they had omitted such a passage, I think they would have done right, it being nothing to the purpose. But on the contrary Mr. Collins, in a letter to Mr. James Gregory the 24th of Decem. 1670, and in another to Mr. Bertet the 21st of Feb. 1671, both printed in the *Commercium Epistolicum*, pag. 505, 507, wrote, that my method extended to second Segments of round Solids. And Mr. Oldenbourg wrote the same thing to Mr. Leibnitz himself the 8th of Decemb. 1674. See the *Commercium Epistolicum*, pag. 517. So you see that Mr. Leibnitz hath accused the committee of the Royal Society, without knowing the truth of his accusation, and therefore is guilty of a misdemeanour. The committee were so far from acting corruptly against Mr. Leibnitz, that they took no notice of his ignorance of geometry in those days, and omitted several other things which made strongly against him; such as were the two letters in my custody, and the paragraph in the preface to the two first volumes of Dr. Wallis's works relating to this matter; and that a copy of Gregory's letter of the 5th of Septemb. 1670, was sent to Mr. Leibnitz in June 1676, amongst the extracts of Gregory's letters.

The committee in their report affirmed, that they had extracted from the ancient letters, letter-books and papers, what related to the matter referred to them: all which extracts, delivered by them to the Society, they believed to be genuine and authentick. Mr. Leibnitz accuses them for not printing the letters entire, including as well what did not relate to the matter referred to them, as what did relate to it: as if it were not lawful to cite a paragraph out of a book, without citing the whole book. Thus he complains, that the *Commercium Epistolicum* should have been much bigger. But when he is to answer it, he complains that it is too big, and would require an answer as big as itself. And so the ancient letters and papers must be laid aside, and the question must be run off into a squabble about philosophy and other matters: and the great mathematician who, in his letter to Mr. Leibnitz, dated the 7th of June 1713, concealed his name, that he might pass for an impartial judge; must now pull off his mask, and become a party-man in this squabble, and send a challenge by Mr. Leibnitz to the mathematicians in England: as if a duel, or perhaps a battle with his army of disciples, were a fitter way to decide the truth, than an appeal to ancient and authentick writings; and mathematicks

must henceforward be filled with achievements in knight-errantry, instead of reasons and demonstrations.

Mr. Leibnitz acknowledges, that when he was in London the second time, he saw some of my letters in the hands of Mr. Collins, especially those relating to Series; and he has named two of them which he then saw, viz. that dated the 24th of October 1676, and that in which he pretends that I confessed my ignorance of second Segments. And no doubt he would principally desire to see the letter which contained the chief of my Series, and particularly that which contained those two for finding the arc by the sine, and the sine by the arc, with the demonstration thereof, which a few months before he had desired Mr. Oldenbourg to procure from Mr. Collins; that is, the *Analysis per æquationes numero terminorum infinitas*. But yet he tells us, that he never saw where I explained my method of Fluxions, and that he finds nothing of it in the *Commercium Epistolicum*, where that Analysis and my letters of the 10th of Decemb. 1672, 13th of June 1676, and the 24th of Octob. 1676, are published. I suppose he means, because he finds no picked letters there. And by the same way of arguing, he and Mr. Bernouilli may pretend, that they find nothing of the method of Fluxions in the introduction to the Book of Quadratures.

He saith also, that he never saw where I explain the method claimed by me, in which he assumes an arbitrary Series. If he pleases to look into the *Commercium Epistolicum*, pag. 530 and 557, he will there see that I had that method in the year 1676, and five years before. And Dr. Wallis, in the second volume of his works, pag. 393, lin. 32, has told him, that this method needs no further explication, than what I there gave of it. Mr. Leibnitz might find it himself, but not so early; and second inventors have no right.

He pretends, that in my book of Principles, pag. 253, 254, I allowed him the invention of the *Calculus Differentialis*, independently of my own; and that to attribute this invention to myself, is contrary to my knowledge there avowed. But in the paragraph there referred unto, I do not find one word to this purpose. On the contrary, I there represent, that I sent notice of my method to Mr. Leibnitz, before he sent notice of his method to me; and left him to make it appear, that he had found his method before the date of my letter; that is, eight months at the least before the date of his own. And by referring to the letters, which passed between Mr. Leibnitz and me ten years before, I left the reader to consult those letters, and interpret the paragraph thereby. For by those letters he would see, that I wrote a tract on that method, and the method of Series together, five years before the writing of those letters; that is, in the year 1671. And these hints were as much as was proper in that short paragraph, it being besides the design of that book to enter into disputes about these matters.

He saith, that when he was in London the first time, which was in January and February 1673, he knew nothing of Infinite Series, nor of the advanced geometry, nor was then acquainted with Mr. Collins, as some have

have maliciously feigned. But who hath feigned this, or what need there was of feigning it, I do not know. At that time Dr. Pell gave him notice of Mercator's Series for the Hyperbola, and he carried Mercator's book with him to Paris, though he did not yet understand the higher geometry. And any of those to whom Mr. Collins had communicated mine and Gregory's Series, might give him notice of them, without his being acquainted with Mr. Collins.

He saith, that after his coming from London to Paris, his first letters were of other matters than geometrical, till Mr. Huygens had instructed him in these things; and that he found the arithmetical quadrature of the circle towards the end of the year 1683, and began to write of it to Mr. Oldenbourg the next year, and found the general method by arbitrary Series a little after, and the *Differential Calculus* in the year 1676, deducing it from the Series of numbers; and that in his letter of the 27th of August 1676, by the words *certa Analysis*, he meant the Differential Analysis. And am not I as good a witness, that I invented the methods of Series and Fluxions in the year 1665, and proved them in the year 1666: and that I still have in my custody several mathematical papers written in the years 1664, 1665, and 1666, some of which happen to be dated: and that in one of them, dated the 13th of Novemb. 1665, the direct method of Fluxions is set down in these words:

PROB. *An equation being given, expressing the relation of two or more lines, x, y, z, &c. described in the same time by two or more moving bodies, A, B, C, &c. to find the relation of their velocities, p, q, r, &c.*

RESOLUTION. *Set all the terms on one side of the equation, that they become equal to nothing. Multiply each term by so many times $\frac{p}{x}$ as x hath di-*

*mensions in that term. Secondly, Multiply each term by so many times $\frac{q}{y}$ as y hath dimensions in it. Thirdly, Multiply each term by so many times $\frac{r}{z}$ as z hath dimensions in it, &c. The sum of all these products shall be equal to nothing. Which equation gives the relation of p, q, r, &c.: and that this resolution is there illustrated with examples, and demonstrated, and applied to problems about tangents, and the curvature of curves: and that in another paper dated the 16th of May 1666, a general method of resolving problems by motion is set down in seven propositions, the last of which is the same with the problem contained in the aforesaid paper of the 13th of Novemb. 1665: and that in a small tract written in Novemb. 1666, the same seven propositions are set down again, and the seventh is improved by shewing how to proceed without sticking at fractions or surds, or such quantities as are now called Transcendent: and that an eighth proposition is here added, containing the Inverse Method of Fluxions so far as I had then attained it; namely, by quadratures of curvilinear figures, and particularly by the three rules upon which the *Analysis per Æquationes numero terminorum infinitas* is founded, and by most of the theorems*

rems set down in the Scholium to the tenth proposition of the book of Quadratures: and that in this tract, when the area arising from any of the terms in the valor of the ordinate cannot be expressed by vulgar analysis, I represent it by prefixing the symbol \square to the term; as if the abscissa be x , and the ordinate $ax - b + \frac{bb}{a+x}$, the area will be $\frac{1}{2}axx - bx + \square \frac{bb}{a+x}$: and that in the same tract I sometimes used a letter with one prick, for quantities involving first fluxions; and the same letter with two pricks, for quantities involving second fluxions: and that a larger tract, which I wrote in the year 1671, and mentioned in my letter of the 24th of Oct. 1676, was founded upon this smaller tract, and began with the reduction of finite quantities to converging Series; and with the solution of these two problems: 1. *Relationem quantitatum fluentium inter se datâ, fluxionum relationem determinare.* 2. *Expositâ æquatione fluxiones quantitatum involvente, invenire relationem quantitatum inter se:* and that when I wrote this tract, I had made my Analysis, composed of the methods of Series and Fluxions together, so universal, as to reach to almost all sorts of problems; as I mentioned in my letter of the 13th of June 1676: and that this is the method described in my letter of the 10th of Decemb. 1672.

In the year 1684 Mr. Leibnitz published only the elements of the *Calculus Differentialis*, and applied them to questions about tangents, and *Maxima & Minima*, as Fermat and Gregory had done before; and shewed how to proceed in these questions without taking away surds, but proceeded not to the higher problems. The *Principia Mathematica* gave the first instances made publick of applying this *Calculus* to the higher problems; and I understood Mr. Leibnitz in this sense in what I said concerning the *Acta Eruditorum* for May 1700, pag. 206. But Mr. Leibnitz observes, that what was there said by him, relates only to a particular artifice *de Maximis & Minimis*, with which he there allowed that I was acquainted, when I gave the figure of my vessel in my Principles. But this artifice depending upon the Differential Method as an improvement thereof, and being the artifice by which they solved the problems, which they value themselves most upon, those of the *Linea celerrimi Descensus*, and the *Linea Catenaria* and *Velaria*, and which Mr. Leibnitz there calls a Method of the Highest Moment, and greatest Extent; I content myself with his acknowledgment, that I was the first who proved, by a specimen made publick, that I had this artifice.

In the year 1689 Mr. Leibnitz published the principal propositions of this book as his own, in three papers, called, *Epistola de Lineis Opticis, Schediasma de resistentia Medii, & Motu projectilium gravium in Medio resistente, & Tentamen de Motuum Cælestium Causis*; pretending that he had found them all, before that book came abroad. And to make the principal proposition his own, he adapted to it an erroneous demonstration; and thereby discovered, that he did not yet understand, how to work in second differences. And this was the second specimen made publick, of applying the

the method to the higher problems. Hitherto this method made no noise, but within a year or two began to be celebrated.

Dr. Barrow printed his Differential Method of Tangents in the year 1670. Mr. Gregory from this method, compared with his own, deduced a general method of tangents without calculation; and by his letter of the 5th of September 1670, gave notice thereof to Mr. Collins. Slufius, in November 1672, gave notice of the like method to Mr. Oldenbourg. In my letter of the 10th of December 1672, I sent the like method to Mr. Collins; and added, that I mentioned it to Dr. Barrow, when he was printing his lectures; and that I took the methods of Gregory and Slufius to be the same with mine; and that it was but a branch or corollary of a general method, which without any troublesome calculation extended not only to tangents, but also to other abstruse sorts of problems concerning the crookedness, areas, lengths, centers of gravity of curves, &c. and did all this, even without freeing equations from surds; and I added, that I had interwoven this method with that of infinite Series; meaning, in the tract which I wrote in the year 1671. Copies of these two letters were sent to Mr. Leibnitz by Mr. Oldenbourg, in the extracts of Gregory's letters, in June 1676; and Mr. Leibnitz, in his letter of the 21st of June 1677, sent nothing more back, than what he had notice of by these two letters; namely, Dr. Barrow's Differential Method of Tangents, disguised by a new notation, and extended to the Method of Tangents of Gregory and Slufius, and to equations involving surds, and to quadratures. But this is not the case between me and Dr. Barrow. He saw my tract of Analysis in the year 1699, and was pleased with it. And before his lectures came abroad, I had deduced the method of Tangents of Gregory and Slufius from my general method. But Mr. Leibnitz in those days knew nothing of the higher Geometry, nor was he acquainted with the vulgar Algebra.

In his letter of the 27th of August 1676, he wrote thus: *Quod dicere videmini plerâque difficultates (exceptis problematibus Diophantæis) ad series infinitas reduci; id mihi non videtur. Sunt enim multa usque adeo mira & implexa, ut neque ab æquationibus pendeant, neque ex quadraturis. Qualia sunt, ex multis aliis, problemata methodi tangentium inversæ.* And when I answered, that such problems were in my power, he replied, in his letter of the 21st of June 1676, that he conceived that I meant by Infinite Series, but he meant by Vulgar Equations. See the answer to this in the *Commercium Epistolicum*, pag. 562.

He saith, that one may judge, that when he wrote his letter of the 27th of August 1676, he had some entrance into the Differential Calculus; because he said there, that he had solved the problem of Beaune *certainly* Analysis, by a certain analysis. But what if that problem may be solved *certainly* Analysis without the differential method? For no further analysis is requisite than this; that the ordinate of the curve desired increases or decreases in geometrical progression, when the abscissa increases in arithmetical; and therefore the abscissa and ordinate have the same relation to one another,

another, as the logarithm and its number. And to infer from this, that Mr. Leibnitz had entrance into the Differential Method; is as if one should say, that Archimedes had entrance into it, because he drew tangents to the spiral, squared the parabola, and found the proportion between the sphere and the cylinder; or that Cavallerius, Fermat and Wallis had entrance into it, because they did many more things of this kind.

P. S. When the committee of the Royal Society published the *Commercium Epistolicum*, the letters and papers in my custody were not produced. Among them were the following letter of Mr. Leibnitz, dated $\frac{7}{7}$ of March 1693, and a letter of Dr. Wallis's, dated the 10th of April 1695; both which, upon a fresh occasion two years ago, were produced, examined, and left in the archives of the Royal Society. The first shews what opinion Mr. Leibnitz had of this matter, before he knew my symbols, or any thing more of the method of Fluxions, than what he learnt from my letters and papers writ in or before the year 1676, or from the *Principia Philosophiæ Mathematicæ*, and by consequence before I could deceive him; and that he then gave me the precedence. The second, compared with the preface to the Doctor's works, shews what opinion the English mathematicians, and some others abroad, had of this matter, when they heard that the Differential Method began to be celebrated in Holland, as invented by Mr. Leibnitz. The first of these two letters, and part of the second, are hereunto subjoined.

‘ Illustri Viro ISAACO NEWTONO

‘ GODEFRIDUS GULIELMUS LEIBNITIUS, S. P. D.

‘ QUANTUM tibi scientiam rerum mathematicarum totiusque naturæ debere arbitrer, occasione datâ, etiam publicè sum professus. Mirificè ampliaveras geometriam tuis seriebus: sed, edito Principiorum opere, ostendisti patere tibi, etiam quæ analysi receptæ non subsunt. Conatus sum ego quoque, notis commodis adhibitis, quæ differentias & summas exhibent, geometriam illam, quam transcendente appellô, analysi quodammodo subicere; nec res malè processit. Sed à te adhuc magni ali- quod expecto ad summam manum imponendam; tum ut problemata, quæ ex datâ tangentium proprietate quæruntur lineas, reducuntur optimè ad quadraturas; tum ut quadraturæ ipsæ, quod valde vellem, reducuntur ad Curvarum rectificationes, ubique superficierum aut corporum dimensionibus simpliciores.

‘ Sed super omnia optem, ut geometricis absolutis, naturam, uti cœpisti, mathematicè tractare pergas; in quo genere certè tu unus cum paucissimis ingens operæ pretium fecisti. Mirificum est, quod invenisti, elipses Keplerianas prodire, si tantummodo attractio, sive gravitatio, & trajectio in planetâ concipiantur. Tametsi enim eò inclinem, ut credam hæc omnia Fluidi ambientis motu sive effici, sive regi, analogiâ gravitatis &

‘ & magnetismi apud nos, nihil tamen ea res dignitati & veritati inventi tui detraxerit.

‘ Quæ summus & ipse Mathematicus Christianus Huygenius in tua novavit, appendice libelli de causâ luminis & gravitatis, expensa tibi non dubito; & sententiam vicissim tuam velim. Vestrâ enim amicâ collatione potissimum, qui in hoc genere eminentis, erui veritas potest.

‘ Cùm verò maximum tu quoque lumen ipsi Dioptricæ intuleris, explicatis colorum phænomenis inexpectatis, velim quid sentias de Huygenianâ explicatione radiationis, utique ingeniosissimâ, cùm feliciter adeo prodeat lex sinuum. Significavit mihi Huygenius, nescio quæ nova phænomena colorum sibi à te communicata. Ego valde optem ut ratio colorum, quos fixos vocant, ex apparentibus deduci possit; seu ut ostendatur ratio efficiendi per refractiones, ut tota aliqua superficies certum colorem ostendat.

‘ In librorum apud Anglos editorum indicibus occurrere mihi aliquoties libri mathematici autore Newtono; sed dubitavi à te essent, quod vellem, an ab alio homonymo.

‘ Heinsius noster redux testis fuit benevolentiae erga me tuæ. De cultu verò meo erga te, non ille tantum testari potest, sed & Stepneius, tecum ejusdem olim Collegii habitator, nunc magnæ Britanniae Regis negotia apud Cæsarem, nuper apud Serenissimum Electorem Brandenburgicum, curans.

‘ Hæc scribo, magis ut studia erga te mea intelligas, quæ nihil tot annorum silentio amittere, quàm ut tua ego studia, quibus auges humani generis opes, interrompere velim vacuis literis & supervacuis. Vale. Dabam Hannoveræ $\frac{7}{7}$ Martii 1693.’

Part of a Letter of Dr. WALLIS'S to Mr. NEWTON, dated from Oxford, April 10, 1695.

‘ I WISH you would print the two large letters of June and October 1676. I had intimation from Holland, as desired there by your friends, that somewhat of that kind were done; because your notions of Fluxions pass there with great applause by the name of Leibnitz's *Calculus Differentialis*. I had this intimation, when all but part of the preface to this volume was printed off; so that I could only insert, while the press stayed, that short intimation thereof, which you find there. You are not so kind to your reputation, and that of the nation, as you might be, when you let things of worth lie by you so long, till others carry away the reputation which is due to you. I have endeavoured to do you justice in that; and am now sorry, that I did not print those two letters *verbatim*.’

The intimation above-mentioned published in April 1695 by Dr. WALLIS in his said Preface.

QUÆ in secundo volumine habentur, in præfatione eidem præfixâ dicitur. Ubi inter alia habetur Newtoni methodus de Fluxionibus, ut ille loquitur, consimilis naturæ cum Leibnitii, ut hic loquitur, *Calculus Differentialis*;

rentiali; quod qui utramque methodum contulerit, satis animadvertat, utut sub loquendi formulis diversis: quam ego descripsi, Algebra, Cap. xci. &c. præsertim Cap. xcv. ex binis Newtoni literis, aut earum alteris, Junii 13, & Octob. 24, 1676, ad Oldenburgium datis, cum Leibnitio tum communicandis, iisdem ferè verbis, saltem leviter mutatis, quæ in illis literis habentur; ubi methodum hanc Leibnitio exponit, tum ante decem annos, nedum plures, ab ipso excogitatam. Quod moneo, nequis causetur, de hoc Calculo Differentiali nihil à nobis dictum esse.

Out of the account given of the works of Dr. WALLIS in the *Acta Eruditorum* for June 1696. Pag. 257, 258.

Cæterum ipse Newtonus—summæ reciprocam. *Vide supra*, p. 568, lin. 16—29.

N. B. IN my letters of the 13th of June and the 24th of October 1676, I affirmed, that I had the method of Fluxions some years before; but I never allowed that Mr. Leibnitz had the Differential Method before the year 1677; nor in those days did I know more of his pretences to it, than what he represented that year in his letter of the 21st of June; nor did I allow the Method of Transmutations to be a general method of Series; nor then knew, that the Series, which he sent to me, was sent to him by Mr. Oldenbourg the year before, and invented by Mr. James Gregory in the year 1671. The Method of Transmutations is not a Method of Series, but a particular theorem for transmutations of figures into one another, like those of Gregory and Barrow. And as for the scholium upon the second lemma of the second book of the *Principia Philosophiæ Mathematicæ*, which is so much wrested against me; it was written, not to give away that lemma to Mr. Leibnitz, but on the contrary to assert it to myself. Whether Mr. Leibnitz invented it after me, or had it from me, is a question of no consequence; for second inventors have no right.

Dr. Wallis, by his letter of Decemb. 1, 1696, printed in the third volume of his works, gave notice to Mr. Leibnitz of the paragraph in his preface to the first volume above cited. And Mr. Leibnitz denied not that in the year 1676, I explained to him the method found by me ten years before or above, nor complained of the Doctor for saying this: *De te autem queri*, saith he in his letter of the 29th of March 1697, *nunquam mihi in mentem venit; quem facile apparet nostram, in Actis Lipsiensibus prodita, non satis vidiſſe*. He allowed, that the methods were of like nature as the Doctor had affirmed; and said, in his letter of the 28th of May 1697, that he therefore called them both by the common name of the *Infiniteſimal Method*; but added, that as the Analysis of Vieta and Cartes were both called by the common name of *Analysis Speciosa*, and yet differed in some things, so my *Infiniteſimal Analysis* and his might differ in some things; meaning some improvements made by him to this method; and excused the Doctor for not mentioning these improvements, because he had not sufficiently seen them in the *Acta Lipsiensia*; and declared that it never came into his mind to complain of him for any thing else.

And

And yet although Cartes never laid claim to the *Analysis Speciosa* of Vieta, Mr. Leibnitz continued amongst his friends to call the Infiniteſimal Method his own, and thereby gave occasion to Mr. Fatio to write what follows:

Out of the tract of Mr. NICHOLAS FATIO DE DUILLIER, intituled, *Investigatio Geometrica Solidi rotundi*, in quod minima fiat resistencia, published in the year 1699.

‘QUÆRET forsan Cl. Leibnitius, unde mihi cognitus sit iste calculus, quo utor. Ejus equidem fundamenta, ac pleraſque regulas, proprio Marte, anno 1687, circa mensem Aprilem & ſequentes, aliisque deinceps annis, inveni; quo tempore neminem eo calculi genere præter me ipsum, uti putabam. Nec mihi minus cognitus foret, si nondum natus eſſet Leibnitius. Aliis igitur gloriatur diſcipulis, me certè non poteſt. Quod ſatis patebit, si olim literæ, quæ inter clarissimum Hugenium meque interceſſerunt, publici juris ſiant. Newtonum tamen primum ac pluribus annis vetuſtiſſimum hujus calculi inventorem, ipſa rerum evidentiâ coactus agnoſco: à quo utrum quicquam mutuatus sit Leibnitius, ſecundus ejus inventor, malo eorum quàm meum ſit judicium, quibus viſæ fuerint Newtoni literæ, aliique ejusdem manuſcripti codices. Neque modeſtioris Newtoni ſilentium, aut prona Leibnitii ſedulitas inventionem hujus calculi ſibi paſſim tribuentis, ullis imponet, qui ea pertractarint, quæ ipſe evolvi, inſtrumenta.’

N. B. Mr. Fatio wrote this as a witneſs. He related what he had ſeen; and his teſtimony is the ſtronger, becauſe it was againſt himſelf, and he was no Engliſhman. He underſtood the methods of us all, and by what he had ſeen and underſtood, he was able to make a true judgment.

FINIS TOMI QUARTI.

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CORRIGENDA IN CONTEXTU.

- Pag. 270, lin. penult. pro *adhærens*, lege *adhærens*.
P. 271, lin. 5, pro *interior*, lege *anterior*.
P. 279, lin. 7, for *CEbc* and *CBbc*, read *Cbac*.
P. — lin. 16, for *AC*, read *Ac*.
P. 280, lin. 23, for *F, G*, read *FG*.
P. 507, lin. 23, pro 170^o, lege 167^o.
P. 530, Ad initium lineæ 23 in orâ libri scribatur *Nº L*.
P. 544, Ad initium lineæ primæ in orâ libri scribatur *Nº LVII*.

I N N O T I S.

- P. 264, lin. 4, pro *Nonchidarum*, lege *Noachidarum*.